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"An Experimental Research upon Cerebro-cortical Afferent and Efferent Tracts." By DAVID FERRIER, M.D., F.R.S., Professor of Neuropathology, and WILLIAM ALDREN TURNER, M.D., F.R.C.P., Demonstrator of Neuropathology, King's College, London. Received May 25,—Read June 17, 1897.

(From the Neuropathological Laboratory, King's College, London.)

(Abstract.)

The primary object of the research has been to elucidate by the aid of destructive lesions, and the study of the consecutive degenerations, the tracts by which impressions of general and special sensibility are conveyed to the cortex of the brain. For this purpose, the cortical area, supposed to be the sensory centre under consideration, was extirpated; and, secondly, the nerve, tract or primary ganglionic structure connected therewith was divided or destroyed.

In this way strands of degeneration were induced, in due course, of cortical afferent or efferent nature, revealed by the osmium-bichromate method of Marchi.

The systems upon which experiments have been performed were :—

(a) The cerebral portion of the *visual system*, consisting of removal of the occipital lobe, extirpation of the angular gyrus, destruction of the pulvinar thalami, and division of the splenium corporis callosi.

The degenerations showed that this portion of the visual system was composed of a corticifugal tract, passing from the occipital lobe, by way of the optic radiations, to the pulvinar thalami of the same side, and to the anterior quadrigeminal bodies of the same and partly of the other side. The angular gyrus has no descending or efferent tract to the basal ganglia, but is connected by means of association

fibres with the superior temporal gyrus, the superior parietal lobule, and the occipital lobe. A system of corticipetal fibres was traced from the optic thalamus to the angular gyrus and the occipital lobe, in which lobe their distribution was as well marked in the external convolutions as in the cuneus and lips of the calcarine fissure.

The angular gyri and occipital lobes are commissurally connected through the splenium and forceps corporis callosi; the callosal fibres having the same cortical distribution as the thalamic fibres.

In this respect our observations are in harmony with those of von Monakow and Vialet.

(b) The experiments upon the *auditory system* consisted of section of the eighth nerve distal, as well as proximal, to the accessory auditory ganglion; destruction of the posterior quadrigeminal body and the internal geniculate ganglion, and extirpation of the superior temporal gyrus.

Inasmuch as the experiments necessitated division of the pedunculus flocculi, the degenerations consequent thereon were first eliminated. These were traced into Deiters' nucleus, the vermis cerebelli, and tegmentum pontis, corresponding with the observations of Bruce and Stscherbach by the myelination method.

The direct connexions of the *vestibular* division, as shown by section of the eighth nerve trunk distal to the auditory ganglion, are with Deiters' nucleus and the tegmentum; while there is also a probable direct connexion with the nucleus of the sixth nerve.

The connexions of the *cochlear* division, forming the central auditory tract, were found to pass from the accessory auditory ganglion by way of the corpus trapezoïdes, in association with the lateral fillet, to the internal geniculate body of the opposite side. Thence a tract was found to ascend to the superior temporal gyrus. This forms the corticipetal or cerebral auditory tract. Degeneration was also traced after destruction of the auditory ganglion into both superior olives, and posterior quadrigeminal tubercles, chiefly of the opposite side. These results are compared with those of Flechsig, Kölliker, &c., obtained by other methods of investigation.

After destruction of the superior temporal gyrus a tract of degeneration was found to descend to the upper part of the pons Varolii, through the outer fifth of the pes cruris. This constitutes the temporo-pontine tract of Bechterew and Déjérine.

The superior temporal gyri are commissurally connected through the forceps of the corpus callosum, and by means of association fibres with the angular gyrus and occipital lobe.

(c) The *cutaneous sensory* and other *corticipetal systems* were studied by the aid of destructive lesions of the tegment of the pons Varolii, crus cerebri, optic thalamus, the posterior

quadrigeminal body and adjacent tegment. Those which specially caused cutaneous anæsthesia were lesions involving the reticular formation of the tegment of the pons and crus.

In some of these cases there was no obvious loss of cutaneous sensibility, while in others this was pronounced. In both cases, however, corticipetal degenerations were induced. These were traced through both limbs of the internal capsule, the external capsule, and the centrum ovale to the cerebral cortex, both on the convexity and mesial aspect, including the gyrus fornicatus. This corticipetal system is less extensive in the frontal than in the other regions of the cerebrum. It would seem to harmonise with the thalamic corticipetal fibres, which Flechsig has recently described as the first, second, and third "sensory" systems, ascending respectively to the Rolandic area, the falciform lobe, the frontal region and gyrus fornicatus, myelinating at different periods.

Many of these fibres of the tegmentum appear to pass through the optic thalamus without ending in it, while others terminate in this ganglion. This is shown by the fact that destruction of the lateral and ventral parts of the optic thalamus led to a more extensive degeneration than that following destruction of the tegmentum alone, the fibres degenerating towards the same cortical regions. But we have not been able to distinguish, by the degenerative method, between those of sensation proper and the other afferent fibres which ascend to the cortex in this region.

Many fibres from the optic thalamus were found to cross by the corpus callosum to the opposite cerebral hemisphere, thus supporting the view of Hamilton that this structure is a decussation as well as a commissure. Our observations show that the decussation is of thalamic corticipetal fibres.

(d) The other *afferent cranial nerves*, which were made the subject of experiment, were the sensory division of the trigeminus, and the glossopharyngeus, which were divided proximal to their ganglia.

Apart from degeneration of the so-called ascending trigeminal and glossopharyngeal roots, traceable as far as the spinal-medullary junction, no evidence was obtained as to their central continuation.

But the symptoms following lesion of the tegmentum cruris placed the sensory fibres from the face in association with those from the body and limbs.

(e) The experiments upon the *prefrontal* and *frontal* areas confirmed the existence of a fronto-pontine tract, which descends through the anterior limb of the internal capsule and the inner portion of the pes cruris to the pons Varolii.

The subjects of experiment were exclusively monkeys.

" Some Observations on the Chemistry of the Contents of the
    Alimentary Tract under various conditions; and on the
    Influence of the Bacteria present in them."* By A. LOCK-
    HART GILLESPIE, M.D., F.R.C.P. (Ed.), F.R.S.E. Communi-
    cated by Professor J. G. MCKENDRICK, F.R.S. Received
    May 18—Read June 17, 1897.

(Abstract.)

    In this paper are given details of experiments which were designed
for the purpose of investigating the grosser chemical changes which
occur in the intestinal tract, and the nature and influence of the
micro-organisms present in the contents after the ingestion of dif-
ferent kinds of food or after the administration of certain drugs.
    The points taken up comprise:—

The reaction of the contents of the alimentary tract in its various
    parts, and the bodies to which this reaction is due.
The amount of chlorine present, and the nature of its combina-
    tions.
The solids present in the contents.
The action of the ferments.
The number and nature of the bacteria.

    A series of experiments were first carried out on a dog with an ileac
fistula, situated at the ileo-cæcal valve. As these experiments did not
prove altogether satisfactory, a further series were made on dogs fed
for some days on special diets, or given certain drugs, and killed
about three hours after their last meal. After removal of the entire
alimentary tract, it was divided into six parts, viz., stomach, duode-
num, jejunum, upper ileum, lower ileum, and large intestine,
double ligatures having been placed between each section. Inocula-
tions were made from these segments with due antiseptic precautions.
Thereafter the contents of each section was analysed.
    A similar experiment was carried out on a calf, and another on the
contents of a portion of the intestines of a man.
    The effects of the following diets were investigated :—

On dogs : 1. *Ordinary*, porridge and milk and some meat.
              2. Porridge and milk.
              3. Boiled beef.
              4. Sterilised milk.

In the calf: Cow's milk. (The calf had not passed the sucking stage.)

The drugs used were: 1. Acidum hydrochloricum dilutum.

                            2. Sodii carbonas.

                            3. Salol.

                            4. Calomel.

                            5. Creasote.

                            6. Benzosol.

                            7. Guaiacol carbonas.

                            8. Ammonii chloridum.

In each instance, two meals were given in the day, and when drugs were used two doses in the twenty-four hours, administered with each meal.

The contents of the ileum were obtained each morning from the dog with the fistula by means of a bag tied over the opening.

The Reaction of the Contents.

1. *Total Acidity.*—The total acidity was estimated in the usual way. In the majority of the observations phenol-phthalein was used as an indicator, with check estimations performed with litmus.

In no instance were the contents of any part of the alimentary canal found to be alkaline.

The total acidity of the contents of the duodenum was almost always above that of the gastric contents, while the acidity present in the large intestine was usually as great as, and ofter greater than, that of the duodenal contents.

On an average the maximum acidity occurred in these sections of the tract, the minimum in the contents of the lower half of the ileum.

A curve formed from the mean values for the acidities in the different sections rises from the figure for the stomach contents to that for the duodenal section, gradually falls until the large intestine is reached, when it rises again abruptly.

The greater the proportion of proteids in the food the higher was the acidity of the stomach and duodenal contents, while the material present in the lower parts of the small intestine was less acid, and the contents of th large bowel of rather higher acidity.

The administration of acid with the food increased the acidity of the contents of the duodenum and large intestine, of alkali decreased it in the stomach, but increased it in the other sections. Compared with the results obtained after the administration of the acid, the alkali caused a marked diminution of acidity in the stomach, but an increased acidity in the small intestine below the duodenum.

Salol differed in its effects from calomel in that it caused no

diminution of the acidity throughout the tract, while calomel was followed by a marked fall in the acidity values obtained from the contents of every section, but especially of the small intestine. Guaiacol carbonate and benzosol increased the acidity of the contents of the ileum in the dog with the fistula.

2. *Acidity after Evaporation at* 100° *C. and Acidity driven off.*—The acidity remaining after drying the fluids at 100° C. was least in the contents of the stomach, most in those of the duodenum when the mean figures for all the observations were analysed. With the exception of the stomach and large intestine the acidities, after evaporation, exceeded those of the original samples. That is to say, more alkali was driven off than acid. This in all probability was due to the breaking up of ammonium lactate by heat and the volatilisation of the ammonia. In all cases the discrepancy between the acidity values obtained with phenol-phthalein and those with litmus varied directly with the difference between the values before and after evaporation. If the acidity was less in amount after evaporation than before, the results as shown by the two indicators coincided; if greater, the value as shown by litmus was less than that arrived at by the use of phenol-phthalein.

This increase of acidity after drying was least after food containing much proteid material. After hydrochloric acid it was marked, and after sodium carbonate very slight. The administration of salol was followed by the presence of free acid throughout the tract, that of calomel by an excess of volatile alkali over volatile acid. Of the other antiseptics tested, creasote acted in a similar manner to calomel but to a less extent, guaiacol and benzosol like salol.

The Acid Factors Present.

In the stomach the acidity was due to hydrochloric acid, either free or combined with proteids, and to a small extent to acid salts. In the duodenum the acidity was caused to a great extent by the presence of hydrochloric acid combined with proteids expelled from the stomach, but also to organic acids, such as acetic and lactic acids. In the lower sections of the tract these organic acids were present along with acid salts, and smaller quantities of other acids belonging to the same series.

Chlorine.—The chlorine contained in the intestinal contents was separated into total chlorine, chlorine driven off by evaporation at 100° C. and chlorine not so volatilised, and which was further separated into a part burnt off at a low red heat, and a part remaining after incineration.

The total proportion of chlorine in the different sections showed a slight decrease from the stomach down to the upper half of the ileum,

and then an increase. This increase in the two lower portions was due to the inorganic chlorides. The chlorine, in combination with organic bodies was found in greatest quantity in the stomach, where it was combined for the most part with proteids. It gradually became less in amount in the intestines, except in the lower ileum, here, however, one observation, in which the contents of that section were very concentrated, caused its average proportion to rise to a high level. Volatile chlorine was detected in the stomach contents, in the duodenum, and other parts of the small intestine. None was found in the large bowel. In the stomach and duodenum much of it was in the form of hydrochloric acid, below the duodenum it was probably present as unstable compounds of ammonia, or of other organic bases formed from proteids by the action of trypsin.

In those instances where the acidity of the stomach contents was high, as after proteid food, the chlorine present in the bowel was present in larger quantity than the average.

Solids.—The total solids at 110° C. were in the largest proportion in the lower part of the ileum. Unfortunately, owing to the large intestine having been emptied shortly before the time when many of the analyses were made, the values obtained for the contents of this section of the canal must be regarded as fallacious. The solids were in lowest proportion in the large intestine, and only slightly greater in the stomach. A much higher figure was obtained from the contents of the duodenum than from the stomach.

The inorganic ash was present in larger proportion in the duodenum, jejunum, and lower ileum than in the other sections.

Bacteriology.

The organisms grown from the contents of the six sections into which the alimentary canal was divided were found to vary in number and character with the degree of acidity present. The higher the acidity the greater was the proportion of acid-forming growths, the majority of which were unable to liquefy gelatine, and, as a rule, the smaller was the total number of bacteria cultivated.

When the acidity was low a greater number of liquefying organisms could be grown, most of which rendered the nutrient medium alkaline in reaction.

When an ordinary diet (porridge, milk, and some meat) was given, the contents of the duodenum yielded the fewest colonies, those of the stomach a few more, while below the duodenum the numbers grown increased progressively. A purely meat diet caused an almost entire disappearance of growths from the tubes inoculated from the stomach, duodenum, and jejunum, a marked fall below the average in the numbers in the ileum, although the colonies obtained from the large intestine were as numerous as usual.

No growths were obtained from the contents of the stomach or jejunum of a dog fed on sterilised milk, and only four from the material in the duodenum. Large numbers were present below this part. The contents of the entire intestinal canal of a calf did not yield a single liquefying organism, while the total number grown was very small, as far down as the lower half of the ileum.

The administration of dilute hydrochloric acid was followed by a decrease in the number of organisms present in each section of the canal, and especially of the organisms capable of liquefying gelatine. Carbonate of sodium, on the other hand, caused no diminution in the numbers found in the upper parts of the tract, but—corresponding to an increase in acidity in the contents of the lower sections— occasioned a diminution of liquefying forms in the ileum and large intestine.

The administration of antiseptic drugs yielded very interesting results. When salol was given, no diminution in the number of organisms was observed until the ileum was reached, when the organisms capable of liquefying gelatine became very few in number. After calomel, the upper sections contained a small number of bacteria, the lower parts a large number which were chiefly of the class able to liquefy gelatine.

No trace of the decomposition products of salol could be detected in any of the sections above the upper half of the ileum. In this portion of the canal only a faint reaction was obtained. In the lower part of the ileum and in the large intestine the reaction was well marked.

The exhibition of other antiseptic drugs was quickly followed by a sensible diminution in the number of the organisms present.

Extract of hæmatoxylin was given for the purpose of testing the action of an astringent on intestinal fermentation. The number of organisms was rather increased during its administration.

Trypsin.

Although the contents of the bowel were always acid, trypsin was found to be active in them. Perhaps this fact may explain why this ferment is secreted for so many hours after food, when pepsin is only secreted during the duration of stay of food in the stomach. Acids, however weak, gradually destroy trypsin; the acid in the bowel must do so, but this destructive action is compensated for by a constant secretion of more of the ferment.

The action of trypsin on proteids in the presence of organic acids was investigated in the dog fed upon sterilised milk.

To 10 cub. cm. of a solution of egg-albumin containing 0·115 gram albumin, 5 cub. cm. of the contents of each section of the

alimentary canal were added. This mixture was rendered alkaline
with sodium carbonate, and then just made acid by the cautious
addition of acetic acid until the neutral point was passed. The
mixtures were then kept at 38° C. for four hours. A check
experiment with liquor pancreaticus in place of the contents showed
that it could digest 68·5 per cent. of the albumin in a slightly acid
solution. The amounts digested by the intestinal contents varied
from 48 per cent. in the case of the lower ileum, 37 per cent. in
the jejunum, to 17 per cent. in the large intestine.

A small proportion of free mineral acid arrested the proteolytic
action of trypsin; a larger percentage of hydrochloric acid com-
bined with proteids was necessary to cause a corresponding degree
of inhibition.

Pepsin.

Artificial digestion experiments, in which 5 cub. cm. of the con-
tents of each portion of the alimentary canal were added to a
mixture containing 0·115 gram of egg albumin and 20 cub. cm. of
decinormal HCl, resulted in a certain amount of the albumin
being digested in each. Seventy-seven per cent. of the proteid was
digested after four hours at 38° C., when 5 cub. cm. of the
stomach contents had been added, 20 to 28 per cent. in the experi-
ments with the duodenal, jejunal, and upper ileac contents, only
12·9 per cent. when the contents of the lower ileum were used, and
19 per cent. when the large intestine was tested.

Distillate of Contents of Bowel.

When the contents of a portion of the intestine were distilled
after the addition of water, if the combined acidity after evaporation
was greater than the total acidity as originally estimated, the first
portions of the distillate were. alkaline and contained ammonia,
even although the contents had been of highly acid reaction.
When the acidity after evaporation was lower than before it, as in
the stomach and large intestine, the distillate was acid from the first,
due, as a rule, to acetic acid. In most cases the acidity of the residue
was found to be chiefly composed of lactic acid.

Proteids.—Coagulable proteids were obtained from the contents
of the lower ileum from the dog with a fistula on each occasion,
varying from 1·92 per cent. to a mere trace. No relation could be
traced between the other factors and the quantity of coagulable
proteid present.

On another occasion half-saturation of the contents with am-
monium sulphate, which precipitates globulins, brought down much
more than the half of the total coagulable proteid present, except
in the contents of the stomach. Albumoses were found in the

stomach in large amount, none in the duodenum or jejunum, and traces in the lower sections of the bowel. Peptones were present in traces in the contents of the stomach and jejunum and in the parts below it.

General Conclusions.

1. The contents of the intestinal canal in the dog and calf, and probably in man, are acid in reaction throughout; the acidity being due to organic acids formed by micro-organisms, hydrochloric acid in combination with proteids and proteid derivatives, and to acid salts.

2. When the food passes from the stomach into the duodenum it rapidly becomes more concentrated from absorption of water, and consequently more acid; it still contains a large proportion of hydrochloric acid in combination with proteid bodies, but the increased proportion of inorganic chlorides indicates that this acid is rapidly being acted on by the soda of the pancreatic secretion.

3. The organisms present in the bowel are divisible into two great groups, those which are able to give the medium in which they grow an acid reaction, and those which cause it to become alkaline or neutral. The first class, as a class, are usually unable to liquefy gelatine. The second class can do so, and form the ordinary putre- factive organisms. These classes are mutually antagonistic. If the number of acid-forming organisms be in large proportion to the total number present, the second class of bacteria fails to grow in any luxuriance, and the intestinal contents do not putrefy in the ordinary sense of the word. *Per contra*, should the liquefying and alkali- forming organisms be in the majority, the first class is less numerous, and intestinal putrefaction may be present. As, however, the diminished acidity which follows the growth and action of the second class of bacteria is favourable for the multiplication of members of the first class, sufficient acid is formed by them in ordinary cases to neutralise the alkali, and generally to cause the reaction to remain acid. The ammonia formed by the second class often unites with the lactic acid produced by the first, creating, in fact, a salt which is very advantageous for the further development of both classes.

4. A normal acidity of the stomach contents with the presence of free HCl, or an increased amount of each of these factors, causes a greater destruction of the alkali-forming or putrefactive bacteria than of the acid-forming and more resistant organisms. This naturally leads to diminished decomposition in the bowel. A diminished gastric acidity, or a large meal chiefly proteid in character, allows a larger number of the second class of bacteria to reach the bowel, and may thus cause intestinal decomposition and indigestion. But such a result is not invariable, as the diminished

acidity of the upper parts of the canal favours, in health at least, the growth of the acid-forming bacteria, and may thus lead to an increased acidity and diminished decomposition in the lower parts of the canal.

5. Some antiseptic substances appear to act more on the first class of organisms than on the second. Thus salol seemed to act more energetically on the liquefying forms than on the acid-forming class, calomel the converse; while salol exerted a greater antiseptic power in the lower part of the intestinal canal, calomel in the upper portions.

6. Trypsin is capable of energetic proteolytic action in the presence of organic acids, but, as it is slowly destroyed by these acids, it has to be constantly supplied in fresh quantities.

7. The figures obtained for the total solids of the different sections show that absorption of fluids is greatest in the duodenum and lower ileum. The absorption from the large intestine can not be compared with the absorption from the other parts owing to the number of times its contents represented the material newly passed from the ileum.

"On a Discontinuous Variation occurring in *Biscutella lævigata*." By E. R. SAUNDERS, Lecturer of Newnham College, Cambridge. Communicated by W. BATESON, F.R.S. Received June 9,—Read June 17, 1897.

The observations recorded in this paper were made upon *Biscutella lævigata*, a cruciferous plant occurring as a perennial herb in the alpine and sub-alpine regions of middle and southern Europe. It was observed by Mr. Bateson that in a valley of the Italian Alps this species exhibits two distinct forms,[*] which exist side by side, the one hairy and the other glabrous. Plants showing various degrees of hairiness, and constituting a series of intermediate forms connecting the two extremes, were also found, but were comparatively scarce. As it may be presumed that in the state of nature the two varieties intercross freely, the question arises—how is their distinctness maintained? For on the supposition that hairiness and smoothness are characters capable of blending freely, it might be expected that offspring derived from a cross between hairy and smooth parents would tend constantly to regress to a mean condition of texture. It was in order to test the validity of this supposition, and to ascer-

[*] Mr. Bateson's attention was drawn to the variations of this species whilst staying in the Val Formazza, for the purpose of studying the alpine forms of the butterfly *Pieris napi*, for it is upon these plants that the variety *bryoniæ* chiefly lays its eggs in this locality.

tain the facts of inheritance, as regards this particular character, that the following observations were undertaken.

Biscutella lævigata has a perennial rootstock bearing a crown of radical leaves.

The leaves are obovate, oblanceolate, or spatulate, tapering downwards to the petiole; the apex is obtuse, and the margin either entire or dentate-sinuate, with a water gland at each marginal tooth or lobe. The cotyledons are obovate, they scarcely taper, and the margins are invariably entire. In those plants in which the more elaborate type of leaf occurs, the first post-embryonic leaves are transitional between this and the simple entire form of the cotyledons.

In respect of flexibility, the leaves of different individuals show very considerable variations. In some plants (and this is more particularly the case with those which are glabrous) they are stiff and brittle and readily crack if the lamina is bent upon itself; in others this bending produces no lesion.

Far more striking, however, are the variations in the character of the leaf surface, which, as stated above, include intermediate gradations between leaves in which both the superficies and the margin are thickly covered throughout their whole extent with rather stiff hairs, and leaves in which the lamina is completely glabrous.

Upon referring to the descriptions of previous observers, I found that several agree in recording the variable character of the leaf surface; as regards the predominance of the different forms, their statements are, however, not altogether in accord.

Rouy and Foucaud[*] describe the leaves of *Biscutella lævigata* as rough and hirsute, rarely glabrous and smooth.

Parlatore,[†] in his simple unqualified statement that the leaves are glabrous or hispid, gives no hint that he regards the latter as the predominant and typical form; in a concluding note he adds that the variations in the degree of hairiness, as of other characters, form such a continuous series, that to consider them of taxonomic value in distinguishing varieties (unless almost infinite in number) would be entirely arbitrary.

Christ,[‡] in contrasting the variety *saxatilis* with the type form (*i.e.*, *lævigata*), in like manner describes the latter as glabrous or pubescent.

Mertens and Koch[§] deal with these variations in greater detail; according to them the leaves may be either thickly covered on both surfaces with rather stiff, spreading hairs, or they may be rough in

[*] ' Flore de France,' t. 2, p. 104.
[†] ' Flora Italiana,' vol. 9, p. 651.
[‡] ' La Flore de la Suisse,' p. 121.
[§] ' Deutschland's Flora,' vol. 4, p. 504.

consequence of tufts of stiff hairs borne on the leaf teeth, or the glabrous character may be even more pronounced, the leaves being destitute of hairs except for a few bristles on the petiole.

Reichenbach[*] and Boreau,[†] on the other hand, make no mention of a glabrous variety; the former describes the leaves as strigose hispid, the latter as pilose or hirsute.

The occurrence of glabrous and hairy varieties within the limits of a single species is not unknown; but in those cases previously investigated it has been found that this divergence of character can be correlated with some sensible inequality in the environment, and that where similar conditions constantly prevail there is uniformity of type. A familiar example of this kind of adaptation is afforded by the well authenticated case of *Polygonum amphibium* which was noticed by Linnæus;[‡] this plant is invariably glabrous when growing in wet ditches or ponds, but it produces leaves more or less downy if for any cause the ponds or ditches dry up. By placing the land variety under appropriate conditions, Hildebrand[§] was enabled to convert it into the typical aquatic form—a sufficient proof that both types are rightly referred to the same species. Nor is this the only instance that can be adduced; Linnæus[||] observed a similar variability in *Plantago coronopus* and other plants; he also records that *Lilium Martagon* assumes a glabrous habit when cultivated in gardens, and that *Thymus serpyllum* becomes more or less hairy when growing on sea-coasts. It was further noticed by Moquin-Tandon[¶] that plants occurring at high altitudes were generally more hairy than these found in the plains. More recently, a large number of comparative experiments have been undertaken by Bonnier,[**] who enumerates a list of species in which, among other changes, a greater or lesser increase in general hairiness resulted from transplantation from valleys to high mountain slopes. Again, Warming[††] states that smooth plants occupying dry areas become hairy in moist situations, and, similarly, those which otherwise are somewhat hairy, under the latter conditions exhibit this character in a more marked degree; he quotes, in support of his statement, *Polygonum persicaria, Ranunculus bulbosus, Mentha arvensis,* and

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[*] 'Flora Germanica Excursoria,' p. 660 ; also 'Icones Floræ Germanicæ,' in which the leaves are figured as hairy all over.

[†] 'Flore du Centre de la France,' t. 2, p. 56.

[‡] 'Philosophia Botanica,' par. 272.

[§] 'Bot. Zeit.,' 1870, p. 20; also Volken's 'Jahrb. des Königl. Botanischen Gartens zu Berlin,' vol. 3 (1884), p. 6.

[||] *Loc. cit.*

[¶] 'Pflanzen-Teratologie,' p. 62.

[**] 'Revue gén. de Bot.,' II, 1890; also 'Ann. des Sci. Nat.,' Sér. VII, t. 20, p. 225.

[††] 'Lehrbuch der ökologischen Pflanzengeographie,' p. 187.

Stachys palustris. Vesque and Viet,[*] in a note to the same effect, also draw attention to an experiment made by Kraus upon the etiolated shoots of the potato, in which he found that those formed in the dark are glabrous when grown in damp air, and downy when the atmosphere is more or less dry.

In the case of *Biscutella lævigata,* however, it seems impossible to explain the variations which occur as the result of a direct modification of habit to habitat—to conceive of them as due to differences in temperature, in illumination, in the humidity of the atmosphere, or in the nutritive capacity of the soil. For in all the instances quoted above, a certain constancy of form is associated with, and characteristic of, a particular environment; whereas in the species in question no such connexion is apparent, the two extreme types are not infrequently found in groups in close proximity to one another, or a hairy and a glabrous plant may even be growing in the same sod of turf, and presumably, therefore, under identical external conditions.

With the immediate cause of these variations, however, I am not here concerned further; whatever may be the active agent in their production, their occurrence suggested that a detailed study of these differences in the leaf surface might lead to interesting results bearing upon the views which have recently been brought forward with regard to discontinuous variation and its value as a factor in the origin of species.

Before entering upon an analysis of the experimental results it will be necessary to consider somewhat more in detail the variations in the degree of hairiness or smoothness exhibited by the leaf-surface. The following types may conveniently be distinguished :—

I. *Surface hairy.*—In the leaves of this class both the upper and the lower surfaces are thickly covered with hairs. In some individuals the hairy character of the leaf surface is easily recognisable at a glance; in others, in which the hairs are short, a closer inspection is necessary.

II. *Surface intermediate.*—To this type belong—

    (*a*) Leaves in which a few stray hairs are scattered thinly over the upper surface.

    (*b*) Leaves in which (more frequently) some portion of the upper surface is quite glabrous. This smooth area usually forms a longitudinal zone of varying breadth on either side of the midrib; the hairs, indeed, may be restricted to the margin and to a narrow belt of marginal surface. In rare cases the hairs are grouped differently : thus, the apical region may be hairy while the basal and middle portions are glabrous.

[*] ' Ann. des Sci. Nat.,' Sér. VI, t. 12, footnote to p. 174.

III. *Surface glabrous.*—This class includes—
      (*a*) Leaves in which the whole upper surface is glabrous,
          except, perhaps, for one or two hairs which, as it were,
          overflow from the continuously hairy margins at the
          level of the leaf-teeth.
      (*b*) Leaves in which both surfaces are glabrous, and, further,
          the marginal hairs are being confined to certain
          definite points—the leaf-teeth.
      (*c*) Leaves in which the lamina is wholly destitute of hairs.

[In types II and IIIa the under surface of the leaf was not
examined, and the scattered hairs occasionally occurring on the
midrib were also disregarded.]

Types I and II are so well marked that it is rare to find a leaf full
grown, and with hairs on the surface, which cannot be referred with
confidence to either category.

The flowering stem is hairy for a longer or shorter distance from
the base in plants belonging to type I; whereas in those belonging to
types II and III it is usually glabrous.

DETAILS OF EXPERIMENTS.

Ripe seeds collected from plants growing in the Val Formazza
were divided into three sets, which were sown respectively in August,
1895, and in February and March, 1896; in addition to this material
a few plants which survived transplantation from Italy, together
with some specimens in the Cambridge Botanical Garden (which
had been raised from seed supplied by M. Correvon, of Geneva), were
also placed at my disposal. The seedlings were raised in pots under
glass, and either planted out in the open in the spring, or repotted
singly for greater convenience of manipulation. A careful examina-
tion of each leaf of the young plants brought to light a fact of con-
siderable interest; it was noticed that in certain cases the first-formed
leaves exactly resembled one another as regards the nature of the
leaf surface, whereas in others the successive leaves exhibited degrees
of hairiness varying more and more widely from the original type.
In order to obtain a record of each leaf, it was found necessary to
take special precautions to protect the plants as far as possible from
the attacks of slugs and snails, to whom the leaves of this species
seem to be especially palatable. The pots were therefore placed upon
cinders, or surrounded with soot; if not thus protected many of the
younger leaves would in a single night be reduced to bare midribs, or
in the case of seedlings, the stump of the stem was often all that re-
mained. The plants with smooth-surfaced leaves were more liable to
attack than those that were hairy, although the latter did not wholly
escape. In one batch of seedlings, which were all placed under the same

shelter, 107 leaves, belonging to 72 different plants, suffered more or less mutilation. Of these the leaf surface was glabrous in 94, intermediate in 5, and hairy in 5; in the remaining 3, so much of the leaf was devoured that its character could not be ascertained with certainty.*

It soon became evident that the individuals in which the uniform character of the leaves was maintained (not only in the seedling, but, as it proved, in the adult stage also) were those which as seedlings conformed to one or other of the extreme types, the leaves being either very hairy, or else destitute of hairs, except perhaps at the leaf-teeth. On the other hand, the plants exhibiting more than one grade of hairiness were those in which the first formed leaves corresponded to one of the types intermediate between these two extremes; among these latter the variations in different individuals were always along the same lines, and consisted in a gradual diminution in the number of hairs in successive leaves, until sooner or later a stationary point was reached, after which the character of the leaf surface usually remained constant.†

In enumerating the various grades of hairiness occurring in a single individual, I have intentionally disregarded the character of the cotyledons, for the reason that they do not appear to bear any constant relation (as regards texture) to the leaves which follow. In the case of a seedling in which as yet only the cotyledons were developed, I found it impossible to predict with certainty the character of the succeeding leaves. It is true that in nearly all hairy plants the cotyledons were also hairy, and that in many that were smooth the cotyledons were almost glabrous; but exceptions to this rule were not wanting, while in intermediates the cotyledons exhibited every gradation between the two extremes. Hence there was always an uncertainty as to whether hairy cotyledons indicated a hairy or an intermediate plant, and whether smooth cotyledons would be succeeded by glabrous or intermediate leaves. That this want of agreement between the cotyledons and the later leaves is a condition not peculiar to this species, but is one of common occurrence, needs no further proof than that furnished by a comparative study of the leaves and cotyledons of other members of the same natural order.‡

* The insufficient protection against such attacks afforded by the marginal tufts of hairs has already been noticed by Stahl ('Pflanzen und Sehnecken,' Jena, 1888, p. 58).

† The stunted leaves which are sometimes formed at the beginning or the close of a period of vegetation, as, *e.g.*, in late autumn or early spring, may prove exceptions to this rule; they are often distinctly less hairy than those of normal size borne on the same individuals. But when active growth once more sets in, the new leaves resemble the type previously established in those that developed normally.

‡ Lubbock, ' On Seedlings.'

The nature and extent of these individual variations will be seen on reference to Tables IV, V, and VI.

Exceptions to the foregoing statements are rare, yet now and again a plant may be found exhibiting an appearance strikingly suggestive of a bud variation. The development of accessory crowns of leaves, sometimes crowded together, sometimes borne on runners, is comparatively common, and as a rule the character of the leaves in such cases is uniform throughout the plant; but in these exceptional individuals the leaves of a secondary crown may differ widely from the original type. In one instance I found a plant bearing a crown of twenty leaves, with the hairs confined to the margin, which had produced a second cluster borne on a runner 3—4 inches long, and composed of eleven leaves, all with a very hairy surface. In another example, a young plant bearing a crown of seventeen leaves, belonging to intermediate types, developed another so close to the first that their leaves intermingled; those of the latter (four in number at the time of observation) were all very hairy.

A few other cases lend themselves less readily to so simple an explanation; in them the tendency to "sport" (if such it be) manifests itself not in a bud but in a single leaf. In such plants a leaf may appear showing a marked access of hairiness which is frequently asymmetrical; so that on one side of the midrib both the upper and under surfaces may be hairy, on the other the hairs may be confined to the margin; or again the hairs may be continuous along the margin of one side, and be entirely absent from the other; in these cases the succeeding leaves show a return to the normal symmetrical type.

Postponing for the present further reference to these exceptional cases, I pass on to a consideration of the results summarised in the following tables.

The total number of seedlings obtained from the three sowings was 280; seventy-two of these were not classified owing to the early death of the plant, or to the want of a continuous record of the leaf character, or in a few cases to the "mixed" character of the plant to which allusion has been made above. The distribution of types among the remaining 208 was as follows :—

Table I.

Analysis of 208 *Plants obtained from Seeds sown in August,* 1895, *and March and April,* 1896.

	No. of plants.	Surface hairy.	Surface intermediate.	Surface smooth.
Sown in August, 1895	47	35	4	8
Sown in February, 1896	59	27	9	23
Sown in March, 1896	102	65	23	14
Totals	208	127	36	45

These numbers are not of value as indicating the relative proportion of the different types occurring in the particular locality from which the seeds were obtained; for in the first place a certain selection was intentionally employed in their collection, and secondly it happened that the majority of the seventy-two plants which were not included in Table I, for the reasons previously stated, belonged to types II (surface intermediate) and III (surface smooth), consequently the proportion of hairy individuals appears to be much larger than it actually was. As a matter of fact it is practically impossible to determine the numerical ratio of the different types in their natural habitat, owing to the characteristic habit previously mentioned of producing accessory crowns of leaves on runners or suckers, a habit which renders actual removal from the soil the only method by which it is possible to ascertain the limits of the "individual."

Of the 208 plants seventy-six were obtained from seeds gathered from two very hairy individuals (A and B); the character of the male parents was unknown, since these two plants like all the others from which seeds were collected had been naturally and therefore possibly promiscuously fertilised. The character of these seventy-six plants is shown in Table II.

Table II.

Analysis of 76 of the 208 Plants included in Table I, being the Offspring of two Hairy Plants, A and B.

	No. of Offspring.	Surface Hairy.	Surface Intermediate.	Surface Smooth.
Plant A	44	39	5	0
Plant B	32	22	8	2
Totals	76	61	13	2

A more detailed analysis of these 208 plants was made in the autumn of 1896, by which time the leaf character had apparently become constant in all those individuals which as seedlings had shown a tendency to assume a more glabrous habit; the results are recorded in the following tables :—

Table III.

Detailed Analysis of the 127 Plants bearing Leaves with a Hairy Surface.

Time of sowing.	No. of plants.	Analysis.		
		Plants dead or with leaves withered or too small for identification.	No. of plants unchanged.	No. of plants which had varied.
August....	35	11	22	2
February..	27	7	20	0
March	65	10	55	0
Totals ..	127	28	97	2

In one of the two individuals which had become smoother, both surfaces of the leaves were now glabrous, but there were numerous marginal hairs. In the other the change was much less pronounced; the under surface of the leaves was destitute of hairs except along the midrib, but the upper surface was hairy except for a very narrow strip bordering the midrib.

Table IV.

Detailed Analysis of the 36 *Plants originally bearing Leaves with an Intermediate Surface.*

Time of sowing.	No. of plants.	No. of plants which had varied.	No. of plants unchanged.	Type of earlier leaves.	Type of later leaves.
Aug.	4	..	2	Surface intermediate	Surface intermediate.
		2	..	,, ,,	,, smooth, hairs on margins.
Feb.	9	2	..	,, ,,	Surface smooth, hairs on margins.
		1	..	,, ,,	Surface smooth, hairs on margins or lobes.
		4	..	,,	Surface smooth, hairs on lobes.
		1	..	,, ,,	Surface smooth, hairs on lobes, or smooth.
		1	..	,, ,,	Quite smooth.
March	23	2	..	,, ,,	Surface smooth, except for 1—2 hairs at the leaf-teeth; hairs on margins.
		8	..	,, ,,	Surface smooth, hairs on margins.
		5	..	,, ,,	Surface smooth, hairs on margins or lobes.
		4	..	,, ,,	Surface smooth, hairs on lobes.
		4	..	,, ,,	Quite smooth.
Totals	36	34	2		

Thus of the thirty-six plants originally bearing leaves with an intermediate surface only two remained unchanged.

These thirty-six plants included the survivors (thirteen) of those descendants of A and B which originally produced leaves with an intermediate surface; the variations occurring in them are shown in Table V.

Table V.

Detailed Analysis of the 13 Descendants of A and B, which originally bore Leaves with an Intermediate Surface.

Number of plants.	Type of earlier leaves.	Type of later leaves.
		Surface smooth, hairs on margins.
11	Surface intermediate.	Surface smooth, hairs on margins.
1	,, ,,	,, ,, hairs on lobes or margins.
1	,, ,,	,, ,, hairs on lobes.
—		
13		

Table VI.

Detailed Analysis of the 45 Plants bearing Leaves with a Smooth Surface.

Time of sowing.	Number of plants.	Type of earlier leaves.	Type of later leaves.
Aug.	1	Surface smooth, except for 1–2 hairs at the leaf-teeth; hairs on margins.	Surface smooth, hairs on margins (many).
	1	Do. do. do.	Surface smooth, hairs on margins (few).
	1	Do. do. do.	Surface smooth, hairs on margins or lobes.
	1	Do. do. do.	Surface smooth, hairs on lobes.
	1	Do. do. do.	Quite smooth.
	1	Surface smooth, hairs on margins.	,, ,,
	1	,, ,, hairs on lobes.	Surface smooth, hairs on lobes.
	1	,, ,, ,, ,,	,, ,, ,, ,, or quite smooth.
Feb.	1	Surface smooth, except for 1–2 hairs at the leaf-teeth; hairs on margins.	Quite smooth.
	2	Surface smooth, hairs on margins.	Surface smooth, hairs on margins.
	3	,, ,, ,, ,,	Surface smooth, hairs on lobes or margins.
	5	,, ,, ,, ,,	Surface smooth, hairs on lobes.
	5	,, ,, ,, ,,	,, ,, ,, ,, or quite smooth.
	5	,, ,, ,, ,,	Quite smooth.
	2	,, ,, hairs on lobes.	,, ,,
March	1	Surface smooth, except for 1–2 hairs at the leaf-teeth; hairs on margins.	Surface smooth, hairs on margins or lobes.
	7	Surface smooth, hairs on margins.	Surface smooth, hairs on lobes.
	2	,, ,, ,, ,,	,, ,, ,, ,, or quite smooth.
	1	,, ,, ,, ,,	Quite smooth.
	2	,, ,, ,, ,,	Surface smooth, hairs on lobes.
	1	,, ,, hairs on lobes.	,, ,, ,, ,, or quite smooth.
Total ...	45		

From the observations summarised in the foregoing tables, it will be seen that the cases in which the whole number of leaves produced

by one individual exhibit a fairly uniform degree of hairiness (or smoothness) are almost invariably those belonging to the extreme types; the plants are either very hairy (type I) or almost glabrous (types IIIc and the smoother forms of IIIb). All the plants in which the leaf surface was originally free from hairs remained smooth, while out of the total number of hairy plants only two varied from the original type. It is those in which the first formed leaves are intermediate in character between these two extremes that the change from a more to a less hairy condition may generally be traced (types II, IIIa, and the hairier forms of IIIb). Consequently we find that the continuous series of gradations from the condition of absolute smoothness to that of extreme hairiness, which may be observed upon examination of the leaves of a large number of seedlings taken at random, is not met with in an equally large and haphazard collection of adult plants; among the latter certain forms have disappeared, and the types which obtain are more sharply marked off from one another. In fact adult plants fall into two groups; the type with leaves with an intermediate surface is not found, or occurs as a rare exception. It follows, therefore, that a census compiled from a set of adult plants, and a similar record obtained from the same individuals before the stationary point has been reached will not give concordant results; in the former case the proportion of plants bearing leaves with a glabrous surface will be higher than in the latter.

In order to ascertain the nature and amount of the variations occurring among the *offspring of unlike parents*, certain individuals which flowered in the summer of 1896 were intercrossed. The plants were placed under muslin covers in order to exclude insects, and the flowers were emasculated while still in bud before the anthers had dehisced, in order to prevent possible self-fertilisation. The seeds thus obtained were sown the same year after having been allowed to ripen for a few weeks; the character of the cross-bred seedlings is shown in Tables VII and VIII.

Table VII.

Classification of 120 Cross-bred Seedlings.

	Surface hairy.	Surface intermediate.	Surface smooth.	Totals.
Number of seedlings derived from five hairy plants × smooth (hairs wanting or confined to the lobes) plants.	4	7	26	37
Number of seedlings derived from five smooth (hairs wanting or confined to the lobes) plants × hairy plants.	5	32	28	65
Number of seedlings derived from one plant, surface smooth, marginal hairs numerous × hairy plant.	12	6	0	18
Totals	21	45	54	120

Table VIII.

Classification of the same 120 Cross-bred Seedlings arranged in Families.

Type of earlier leaves.	Offspring of five hairy plants fertilised by very smooth plants.					Offspring of five very smooth plants fertilised by hairy plants.					Offspring of one *rather* smooth plant fertilised by hairy plant.
	Plant 1.	Plant 2.	Plant 3.	Plant 4.	Plant 5.	Plant 1.	Plant 2.	Plant 3.	Plant 4.	Plant 5.	
Surface hairy	—	—	1	—	3	2	—	—	—	3	12
Surface intermediate.	1	3	1	1	1	12	—	6	8	6	6
Surface smooth, except for 1–2 hairs at the leaf teeth; hairs on margins	3	4	1	2	—	2	—	3	4	2	—
Surface smooth, hairs on margins	7	8	1	—	—	2	1	10	2	2	—
Surface smooth, hairs on lobes	—	—	—	—	—	—	—	—	—	—	—
Quite smooth	—	—	—	—	—	—	—	—	—	—	—
Totals	11	15	4	3	4	18	1	19	14	13	18

In all the " very smooth " plants used as parents in these experiments, the leaf-surface was quite smooth, and if marginal hairs were present they were confined to the leaf-teeth. In the " rather smooth " plant there were numerous hairs on the margins and leaf-teeth.

From the observations made upon this one generation of crossbreds, it would appear that when the extreme forms are intercrossed the offspring seldom exhibit the degree of hairiness characteristic of the more hairy parent; in most cases the first formed leaves corre-

spond to one of the glabrous or intermediate grades. In the one
instance in which a plant not belonging to the extreme smooth type,
but with numerous marginal hairs, was crossed with a hairy form,
the general level of hairiness in the offspring was very considerably
higher.

At the time of writing the character of the twenty-one hairy
plants was unchanged. The fifty-four plants bearing leaves with a
smooth surface were still smooth, and the number of marginal hairs
was gradually becoming less. The remaining forty-five plants
originally bearing leaves with an intermediate surface were all
tending to become less hairy, in fact the leaf surface was free from
hairs in all those which had apparently reached the stationary
point.*

A few experiments were also made with the view of determining
the character of the offspring in cases in which the parents *resembled
one another in texture*. To this end certain individuals were placed
under muslin covers, and either self-fertilised or crossed with others
of the same type. Unfortunately none of the plants that were
smooth set seed; four hairy individuals of the Genevese stock,
however, fruited freely, and from them sixty plants were obtained,
all of which showed the same degree of hairiness as the parents.
It is of interest to compare these numbers with those obtained from
the two hairy plants A and B (see Table II), which were freely
exposed to insect visits; in the case of the latter only about 80 per
cent. of the offspring belonged to the hairy type.

Although the results tabulated in the preceding pages have been
obtained from observations upon a comparatively small number of
plants, they are, I think, sufficiently concordant to justify the follow-
ing conclusion. The experiments went to show that a blending of
parental characters as regards hairiness and smoothness occurs to a
certain extent in the offspring of plants of dissimilar types, giving
rise to intermediate forms. But this intermediate condition in
respect of hairiness is only found exceptionally among full-grown
individuals. For whereas in plants which at first are *distinctly hairy*,
the hairiness persists almost without exception, I have found that
in nearly every case those plants which as young seedlings present
an *intermediate* condition, assume, as they grow older, a more dis-
tinctly glabrous habit. This change of character in the cross-bred
seedlings which are originally intermediate, takes place gradually;
occasionally it does not occur until several leaves have been produced,
but more often it is apparent as soon as the second and third leaves
have developed.

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* Though the seeds were all sown together, they germinated at such unequal
intervals that the plants were at this time in very different stages of development.

HISTOLOGICAL NOTE.

A microscopical examination of the leaves revealed the presence of a histological feature of some interest. The general arrangement of the tissues follows the normal dorsiventral type; it is the mesophyll, however, which claims especial attention, and exhibits a structural peculiarity which consists in the *thickening* and *lignification* of the *cell walls* in such a way as to form a latticed network. The meso- phyll cells of the cylindrical leaves of some species of *Sansevieria* exhibit a somewhat similar appearance as has been previously recorded by De Bary,* and figured by Henfrey.† These bands of thickening give the characteristic lignin reaction with Schulze's solution and with phloroglucin. In all respects, save for this modification of the walls, these cells resemble the rest of the mesophyll, and judging from the amount of chlorophyll contained in them, their capacity for amylogenesis is not less than that of the unaltered cells. The number and distribution of these cells varies considerably in different cases, but so far as I have been able to ascertain, their occurrence is not correlated with any other structural feature. In order to determine their presence or absence in any given case, the leaves to be examined were allowed to rot in water until the epidermal layer could be easily peeled off with forceps from the under surface; they were then mounted whole, with the under surface uppermost. When sufficient transparency could only be attained by decolorisation, the leaves were placed in alcohol, and afterwards boiled for a few minutes in water or in dilute HCl, to facilitate the removal of the epidermis. The method of examination by sections was found to give unreliable results, since the distribution of the cells is so erratic that their non-occurrence in a number of sections affords no certain criterion of their absence from the whole leaf. They may occur singly and remote from one another, or in groups in the meshes of the fibro-vascular network; or continuous layers of the spongy mesophyll or of the palisade cells may undergo this modification; in one case they may cover an un- interrupted area almost as large as the leaf surface, in another they may perhaps be confined to one side of the midrib, and be wholly wanting on the other, in fact, their distribution would appear to be entirely haphazard. In some cases they are present in the cotyle- dons, but more often they are absent from these organs, though they may be present in the later-formed leaves of the same plants; their presence in one leaf does not necessarily imply their occurrence in other leaves of the same individual. To take a single instance—nine leaves belonging to a very hairy plant were examined; in five of these the cells were abundant everywhere; in two a few were present; and

* 'Comparative Anatomy of Phanerogams and Ferns,' p. 118.
† 'Elementary Course of Botany,' p. 483 (2nd edition).

in the remaining two they were absent altogether. Besides those of *Biscutella lævigata* I examined the leaves of four other sub-species (?)—viz., *B. raphanifolia,* *B. ambigua,* *B. lyrata,* and *B. auriculata,* but I failed to discover any indication of lignification in the mesophyll of these plants.

Addendum, July 28, 1897.

Since the foregoing paper was communicated to the Society, I have myself visited the Val Formazza, and examined the character of the Biscutella plants in this locality, and also in one or two Swiss valleys. The results of these observations are given below :—

Val Formazza, from the head of the valley down to the Tosa Falls (5500 feet).

In this reach of the valley the plants are abundant everywhere—on the banks of shingle and sand and in the low-lying meadows near the stream, and on the grassy slopes of the surrounding heights, up to the Val Toggia on the one side, and as high as the lower limit of the Hohsand Glacier on the other. Both the hairy and the glabrous types were found, each variety often forming patches of varying size; such groups of dissimilar plants may occur side by side on exactly similar ground; or, on the other hand, a small area may be occupied by both forms, which are indiscriminately mixed together. On the whole the smooth individuals were more numerous than the hairy, especially in the low-lying meadows near the river, on the steep slope up to the Val Toggia, and on the slopes on the opposite side of the river between the chálets of Morasco and Riale. In the above-mentioned meadows intermediate plants were also found, especially the smoother forms, and though very few in number compared with the extreme types, they were more numerous here than in any other locality which I had the opportunity of observing. Many of these intermediates were apparently young plants, and their comparative abundance in this spot may, I think, be explained by the fact that the season was a late one, and that consequently some individuals were ranked as intermediates, which had not yet reached the stationary point, and which would eventually conform to the smooth type.

Val Formazza below the Falls and at Al Ponte (4200 feet).

Here the plants were much fewer in number than in the upper reach of the valley; immediately below the falls they were almost all smooth (no intermediates were seen); at Al Ponte the hairy and the glabrous types were found together on the shingle near the stream.

Val Bedretto (All' Acqua, 5265 feet).

The plants were exceedingly abundant in this part of the valley, both on the lower grass slopes and close to the stream; in both places the great majority belonged to the hairy type. Intermediates of the more hairy kind occurred here and there, generally in patches. The very smooth type was not common.

Val Canaria (Airolo, about 3900 feet).

Here the plants, which were only moderately abundant on the grass slopes, were all hairy.

Valley of the Rhone (at the foot of the glacier).

A few plants were growing on the shingle in the river bed, all very hairy.

Valley of the Rhone (Ulrichen, 4380 feet).

Only a very few plants were found, all very hairy.

Valley of the Rhone (Eginen Thal).

Plants numerous, both glabrous and hairy occurring together; a few of the hairier forms of intermediates were also found.

Val d'Anniviers and neighbourhood of Berisal (Simplon).

According to Mr. Bateson's observations in the preceding year Biscutella plants were abundant in both these localities; in the former all the plants were very hairy, in the latter the hairy type predominated, but some hairy intermediates were also found.

"Studies in the Morphology of Spore-producing Members. Part III. Marattiaceæ." By F. O. BOWER, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. Received May 27,—Read June 17, 1897.

(Abstract.)

The memoir, of which this is an abstract, deals with the sori of all the four living genera of Marattiaceæ; the development has been traced in *Angiopteris* and *Marattia* from the earliest stages to maturity, in *Danæa* and *Kaulfussia* from such early condition as the material would permit. Some of the results from *Danæa* have been already submitted to the Society in a preliminary statement.[*] One result of the investigation has been to demonstrate, as regards their development, the substantial unity of type of the sporangia in the four genera. In all of them a single "superficial parent cell" of prismatic form is to be recognised embedded in the massive sporangium when young, not in a central position, but directed obliquely

* 'Roy. Soc. Proc.,' vol. 59, p. 141.

towards the centre of the sorus. By periclinal division this forms internally the archesporium, externally that part of the wall where dehiscence takes place. The tapetum arises, typically in them all, from the cells surrounding the archesporium. The dehiscence is in all by a slit in a radial plane, which may widen to a circular pore in *Danæa*. In those sori where the sporangia are united laterally there is no annulus; it is present only where the sporangia are separate, as in *Angiopteris*.

An interesting feature is disclosed by estimates of the potential spore-production of the single average sporangium in the four genera; the results in round numbers are, in *Angiopteris* 1,450, in *Danæa* 1,750, in *Marattia* 2,500, in *Kaulfussia* 7,850. It is to be remembered that the usual numbers in Leptosporangiate ferns are 48—64; in some Leptosporangiate ferns (*Osmunda*) the number may rise to 500. I have ascertained in *Gleichenia*, however, that the number may be as high as in *Angiopteris*. This large potential output of spores goes parallel with the broad base of the sporangia; in fact, the Eusporangiate condition is that best adapted for maturing large numbers of spores in the individual loculus.

Frequent deviations from the type have, however, been observed, as well as variations of size and mode of segmentation of the sporangia, and it is not possible in certain cases to refer the whole sporogenous tissue of one sporangium to a single parent cell. A special study of the irregularities has been made in *Danæa*, in which genus they are most marked; incomplete septa are frequent, and the sporangia are of very unequal size. The main features have already been noted in the preliminary statement on that genus, where it has been pointed out that comparison of the details with those of the septate anthers of some Angiosperms shows that there is a remarkable resemblance between the two cases. Similar irregularities have been noted, though less commonly, in *Kaulfussia*, and *Marattia*, and rarely in *Angiopteris*.

Those fossil Marattiaceæ which are best known as to the details of the sorus have been compared, and the substantial similarity of the sori in certain cases to those of the modern genera recognised. The facts from fossils and from the modern Marattiaceæ have been made the basis for a fresh discussion of the theoretical question, whether the synangium is or is not a result of coalescence of sporangia? It is concluded that the palæophytological evidence leaves the question open as to the priority of existence of forms with synangia, or with separate sporangia, in the Marattiaceæ. Notwithstanding that writers of authority have treated the question as decided, that the synangia are a result of fusion of distinct sporangia, it is held with some persistence that it is still open; the palæophytological evidence is inconclusive, while the comparative evidence from the living

genera will not only accord with, but appears actually to support a view of septation.

For the analogy with septate anthers, where septation must have occurred, and the similarity between the details of these and those in *Danœa*, and especially the partial septations in both, make it appear probable that in this genus progressive septation has taken place. It is thought probable that progressive septation has been a feature, at least where the sori are elongated, as in *Danœa*. But the question is left over for future discussion whether or not a similar septation, rather than coalescence, may be accountable also for the origin in the first instance of a circular sorus with a plurality of sporangia united together as in *Asterotheca*, or in *Pecopteris unita*.

"On the Development of Marsupial and other Tubular Enamels, with Notes upon the Development of Enamel in General." By CHARLES S. TOMES, M.A., F.R.S. Received July 12, 1897.

(Abstract.)

It was pointed out by my father, the late Sir John Tomes, that the enamel of marsupials was peculiar in that in the whole class, with the solitary exception of the Wombat, the enamel is freely penetrated by tubes which enter it from the dentine, and are continuous with the dentinal tubes at the junction of the two tissues. This character is met with sporadically in other mammals—for example, in the Jerboa among rodents, in the Shrew among insectivora, and notably in the Hyrax, in which animal the free penetration makes its enamel look quite like that of a marsupial.

Whilst there is a large literature upon the development of ordinary enamel, little or nothing has been written about that of tubular enamels.

The outermost portion of marsupial enamel is always devoid of tubes, and the extent to which the tube system exists varies greatly in different members of the group, so that the same enamel organ is obviously capable of forming either tubular enamel or enamel with solid prisms. Moreover, the sporadic reappearance of tubular enamels amongst mammals who have for the most part lost this character, and its occasional occurrence in a rudimentary condition as an abnormality in man, point to its not originating in any manner fundamentally different from that of ordinary enamel development; and it is claimed that the study of its development in marsupials affords the clue to the real nature of enamel development in all animals.

The nature of the question renders it impossible to convey in brief space the grounds upon which the conclusions have been arrived at, but they are—

That the special cells of the enamel organ (Ameloblasts) do not themselves calcify.

That they each furnish from their free ends outgrowths or processes which are continuous with their own plasm, and which may be traced through the entire thickness of young enamel.

That one ameloblast furnishes the whole length of an enamel prism.

That the fibrillar outgrowths, previously more or less correctly described by other observers in other enamels, but apparently not appreciated at their full importance, do calcify from without inwards in such a manner that an axial canal is left uncalcified. Hence the canals of marsupials are in the centres of the prisms, and not, as supposed by Von Ebner, in the interspaces of the prisms.

And that towards the completion of the full thickness of the enamel the central axis is no longer left soft, but the whole calcifies into a solid prism.

It is claimed that other enamels, for instance human enamel, calcify in the same way. It has long been known that short processes hang out from the ends of the ameloblasts, and these, having first been described by my father, are generally styled Tomes' processes; and also that the earliest formed layer of enamel is perforated, so that acids will peel up a perforated membrane from its surface during its development. Longer fibrils have also been detected by Andrews, Williams, and others; but so small a thickness is occupied by these structures, and the full solidification of the prism follows so close upon the heels of any change in the direction of calcification, that the true nature of these structures has not been detected.

But in marsupial enamel, owing to the tubular condition which is so very transient in human and other mammalian enamels being permanently retained, the problem is presented under conditions more favourable for elucidation.

Hence it is my belief that all enamels alike are formed by the centripetal calcification of fibres furnished by the ameloblasts, and that tubular enamels are nothing more than the perpetuation of a stage which is passed through, though only for a brief period, by every solid enamel prism. This view serves to explain the occurrence of the various forms of tubular enamel which are found in fish, in some of whom— *e.g.*, Sargus—the reverse order of things is met with—that is to say, the prisms first formed near to the dentine are solidly calcified, but as their growth goes on the later-formed portions become tubular, so that in the completed enamel there appears to be a system of tubes entering it from its free surface.

In certain cartilaginous fish there is a combination of both of
these arrangements of tubes, from the dentine and from the sur-
face, and sundry other apparently anomalous conditions are met
with.

But if the views advocated in this paper be accepted, all diffi-
culty in accounting for these arrangements, very difficult to explain
from any teleological standpoint, disappear, for they become merely
slight variations or arrests at different stages of a process common to
all enamels during their formation.

"On a Green Leucocytosis in Oysters associated with the
     presence of Copper in the Leucocytes." By RUBERT
     BOYCE, M.B., Professor of Pathology in University College,
     Liverpool, and W. A. HERDMAN, D.Sc.. F.R.S., Professor of
     Zoology in University College, Liverpool.   Received
     July 9, 1897.

In the course of an investigation upon oysters under normal and
abnormal conditions, upon which we have been engaged for the last
two years, and upon which we propose to submit to the Society a
detailed memoir during next session, we have come upon a pheno-
menon which we regard of such considerable importance that we
desire to publish a brief record of our observations and experiments,
as we believe they may prove of interest to other biologists who are
engaged in work on the micro-chemistry of the cell.   The phe-
nomenon we have now to describe is the presence of large quantities
of copper in certain green leucocytes found in a diseased condition
of the American oyster.   The oysters suffering from this leucocytosis
are always more or less green, but must not be confounded with
ordinary green gilled oysters, where the colour is due to a totally
distinct cause.

History.

Green oysters have been known from an early period, and there
are various historic cases on record[*] of people having been poisoned
by eating green oysters, and of the oyster merchants being put upon
trial because of the deleterious nature of their goods.   Periodically
green oysters have been suspected or convicted of being coloured
with copper, and just as often it has been proved by competent
authorities that copper has nothing whatever to do with the green
colour.   This difference of opinion in the past has undoubtedly been

* An interesting historical survey of the subject up to 1866, was given by the
late Mr. Arthur O'Shaughnessy, in the 'Annals and Mag. Nat. Hist.,' ser. 3,
vol. 18.

largely due to the fact that the observers worked with different kinds
of oysters. Some have investigated the celebrated "Huîtres de
Marennes" (a form of *Ostrea edulis*), and have found that while
having dark blue-green gills, they were still in a perfectly healthy
state, that they contained very little copper, and that some iron was
present in the pigment. All that is perfectly correct, but it does
not enable us to draw any conclusions in regard to other green
oysters. There are evidently several kinds of greenness in oysters,
and whereas some may be due to normal and healthy processes, others
must be regarded as abnormal or diseased conditions. It is the
latter, in our experience, that contain the copper.

As early as 1835, Bizio showed that certain oysters he obtained at
Venice contained copper, and he attributed (1845) their bluish-
green colour, and that of the Marennes oyster, to the presence of
that metal. Two subsequent discoveries have thrown a certain
amount of probably undeserved discredit upon Bizio's work. These
are (1) the determination by Fredericq and others that a certain
small amount of copper is present normally in the hæmocyanin
of the blood of crustaceans and molluscs; and (2) the excellent
work of Lankester* and others on the Marennes oysters which
established the normal, healthy condition of the greenness, and the
absence in that form of any copper beyond the trace due to hæmo-
cyanin. We now think it very probable, in the light of our recent
experience, that Bizio was dealing, in the case of his Venetian
oysters, with the same copper-bearing green pigment that we have
met with.

About 1880 Ryder investigated some green oysters in America,
and from his description of what he found we cannot doubt that he
had before him the same kind of green American oyster (*Ostrea
virginica*) that we have been examining. He showed that the green
colouring matter was taken up by the amœboid blood cells, and that
these wandering cells containing the pigment were to be found in
the heart, in some of the blood vessels, and in aggregations in
"cysts" under the surface epithelium of the body. He describes
the colour (in the ventricle) as a "delicate pea-green," and states
that it is not chlorophyll nor diatomine: he suggests that it may be
phycocyanin or some allied substance.†

So far as we are aware there has been no work‡ since Ryder's,

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* See Professor Lankester's memoir on "Green Oysters," in the 'Quart. Jour.
Micro. Sci.' for 1886, which gives an excellent discussion of the subject so far as
the Marennes oyster is concerned.

† Ryder's papers are in the 'U.S. Fish. Commission Reports and Bulletins'
rom 1882 to 1885.

‡ Except Carazzi's passing allusions to our work in 'Mitth. Zool. Stat. Neapel'
for 1896. His own investigations were made upon other kinds of oysters.

dealing with what we described a couple of years ago as the green leucocytosis in the American oyster.   Many papers, to which we do not refer, have appeared dealing with other kinds of green oysters, but they do not affect our present subject.

In January, 1896,* we referred briefly to what appeared to be an inflammatory condition, accompanied by a pale chalky-green colour, which we found in some American oysters relaid at Fleetwood, on the Lancashire coast; and at the Liverpool meeting of the British Association, last September, in discussing various kinds of greenness in oysters, we referred to this diseased condition, in the following terms :—

"There is, however, a pale greenness (quite different in appearance from the blue-green of the " Huîtres de Marennes ") which we have met with in some American oysters laid down in this country, and which we regard as a disease.   It is characterised by a leucocytosis, in which enormous numbers of leucocytes come out on the surface of the body, and especially on the mantle.   The green patches visible to the eye correspond to accumulations of the leucocytes, which in mass have a green tint.   These cells are granular and amœboid.   The granules do not give any definite reaction with the aniline stains, and, so far, we have not made out their precise nature."

Finally, towards the end of last year, in the 'Supplement to the Twenty-fourth Annual Report of the Local Government Board,' Dr. Bulstrode corroborated our statement as to the presence of the pale green disease from the examination of specimens from Truro and Falmouth.   Dr. Thorpe stated in the same Report that he had found that some green oysters from Falmouth,† sent to him for examination by Dr. Bulstrode and Mr. R. Vallentin, contained notable amounts of copper, in some cases as much as 0·02 grain per oyster, while the amount normally present is only 0·006 grain.

Dr. Charles Kohn has kindly, during the last year or so, made a number of analyses for us of different kinds of oysters—"natives," "Marennes," Dutch, and American—and whereas in most of these he has found the copper to be present only in small quantities, on the average agreeing well with the amount (0·006 grain) usually stated as present in the tissues of the normal healthy oyster, in some green Americans which we gave him recently for the purpose he has found a very much larger amount of copper.   These circumstances induced us to reopen, in our investigations, the question of copper in certain green oysters, with the results that are detailed below.

* Report for 1895 on the Lancashire Sea-Fisheries Laboratory (' Trans. Biol. Soc.,' Liverpool, vol. 10, p. 158).

† Obtained from a creek, which is locally supposed to bring down copper, and the water of which was found on analysis to contain some copper.

The Green Leucocytosis.

We first noticed this diseased condition in the autumn of 1895, in some ordinary American oysters (" blue points "), belonging to the species *Ostrea virginica*, which had been imported into Liverpool and relaid near Fleetwood, in the estuary of the Wyre. Since then many hundreds (probably several thousands) of American oysters have been examined by us, and we have seen all degrees of the leucocytosis. It manifests itself in patches and streaks of green on the mantle and other parts of the integument, in engorgements of the blood vessels, especially of those that ramify over the surface of the viscera, and in masses of green-coloured leucocytes in the heart. This green condition, although much less frequently seen in " natives " (*O. edulis*), is occasionally met with there also, and we have recently had some specimens from Falmouth with very well-marked green hearts, due to an accumulation of leucocytes laden with green granules in the ventricle. Such hearts are of frequent occurrence in the diseased American oysters; after death the mass of leucocytes subsides to the bottom of the cavity, leaving the clear plasma above. It is thus easy to demonstrate that the colour is due to the leucocytes, and to the leucocytes alone.

The blood of these oysters contains a great variety of more or less colourless and more or less green and granular corpuscles, all of which may be termed leucocytes. They are apparently all amœboid wandering cells, comparable to the colourless corpuscles of the blood of higher animals. The larger and (probably) older of the leucocytes are very coarsely granular and very opaquely green. It is these that give the colour in bulk. We find them in masses in the heart, in both auricle and ventricle, in the vessels, where they are sometimes so abundant as to engorge or inject certain parts of the system, in the lacunar spaces of the connective tissue of the mantle and other organs, and also in the more solid parts of the tissues wandering amongst the other cells, wedged into the epithelium and coming out in great numbers on the surface of the body. Some of these latter, when found in the ectoderm and on the surface, are very markedly eosinophilous; those in the vessels are not so markedly so. When stained with osmic acid the granules of the leucocytes become black. After treatment with fat solvents, however, some of the leucocytes are still very granular.

In sections which have not been stained, the granules of the leucocytes have a distinctly brown colour, recalling the appearance of the granules in the liver cells in unstained sections in cases of pernicious anæmia.

We opened many batches of American oysters, 100 at a time, and in all cases where the green tint was present in the mantle, heart, or

vessels we found the accumulations of leucocytes. From 120 oysters
we chose the six greenest and the six whitest. Dr. Kohn analysed
these two sets of six for us, and found that the green contained
between three and four (3·7) times as much copper as the white. This
shows that it is not merely a redisposition in the body of the copper,
due possibly to the hæmocyanin, but that there is an absolute increase
in the amount present in the body.

We also found that the greenest parts of the body contained far
more copper than corresponding tissues which had no green deposit
in them. Not only then do these green oysters contain a largely
increased amount of copper, but we have also shown that the copper
coincides in its distribution with the green leucocytes, and, conse-
quently, we regard the copper as the cause of the green colour. We
then passed on to a more minute examination of the pigment and to
histo-chemical reactions.

Chemical Reactions.

The Green Colouring Matter.—The greenest portions of the green
oysters were snipped out and dried on the water bath. The dried
powdered residue was treated with alcohol, ether, chloroform,
benzene, turpentine, xylol, but these reagents failed to extract the
colouring matter; we concluded, therefore, that the pigment was not
of the nature of a lipochrome. On the other hand, the pigment was
readily soluble in dilute acids and in alkalis; the addition of am-
monia gave rise to a distinct bluish tint, and fresh pieces of the green
oysters reacted instantly with ammonia, with the formation of a
beautiful blue.

We next determined whether the pigment was due to iron or
copper. The dried residue treated with dilute hydrochloric acid
and potassic ferrocyanide gave a marked red reaction, thus indi-
cating the presence of copper, and it was then found that very
small quantities of the green colouring matter treated with dilute
hydrochloric acid were sufficient to produce a well-marked deposit
of metallic copper upon polished iron. In several instances a
deposition of copper occurred when a piece of polished iron was
laid upon a green patch on the surface of the mantle of a fresh
oyster, dilute hydrochloric acid having been previously used to
moisten the mantle. Control experiments were made with the
whitest portions of the American oysters and with natives, and
traces only of copper were found. These results have been also
quantitatively controlled by Dr. Kohn.

A series of histo-chemical reactions were then carried out. For
the purpose the oysters were hardened in absolute alcohol, and
pieces were then imbedded in paraffin, great care being taken that

every reagent was perfectly pure, firstly, with regard to the absence of copper or iron, and, secondly, that no acid was present; thus, for example, commercial turpentine may give a distinctly acid reaction, and this would be sufficient to remove the copper. If sections were imbedded in gum—and often the best results were obtained by this method—the tissues were allowed to remain for as short a time as possible in distilled water and then transferred to perfectly fresh neutralised solution of gum-arabic, and allowed to remain in it for only a short period. The pigments appeared partially soluble in water.

Comparatively thick sections were cut, in which the distribution of the green colour could be seen with the naked eye. These were placed in absolute alcohol in every case before proceeding to test. The reagents which we employed were potassic ferrocyanide, 1·5 per cent. solution,[*] freshly prepared ammonium-hydrogen sulphide, and pure hæmatoxylin.

Potassic Ferrocyanide.—Sections were taken from absolute alcohol and passed into distilled water for a moment in order to remove the alcohol. They were then placed in the potassic ferrocyanide solution, when the portions previously green assumed a red colour; this reaction set in immediately. The presence of a 0·5 per cent. solution of hydrochloric acid added in equal quantity to the ferrocyanide solution previous to use (as recommended by Macallum for iron) tended to hasten the reaction, and in some cases was necessary in order to obtain it.

The sections were then washed in distilled water, dehydrated in absolute alcohol, cleared in cedar oil, and mounted in Canada balsam. The red coloration was found located to the masses of leucocytes, and the individual leucocytes themselves were of a faint yellowish-red colour. In the cases of the very granular pigmented leucocytes the granules assumed a distinct red-brown colour. In this way the distribution of the leucocytes and of the vessels which contained them was mapped out. Very beautiful preparations of the engorged green vessels were obtained by partially dissecting the mantle in the fresh oyster so as to expose the ramifying vessels, then hardening in alcohol, and subsequently treating with ferrocyanide solution, when the vessels assumed a well-marked red colour; beautiful results were also obtained by ammonia. Fresh blood obtained from the heart in which vast numbers of the green leucocytes were present also gave a red reaction with acidulated ferrocyanide solution. Control bloods from white oysters gave an exceedingly faint or no reaction.

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[*] For the sake of uniformity we finally adopted the strength of solutions given by Macallum in his paper on the "Distribution of Assimilated Iron Compounds," 'Quarterly Journal of Microscopical Science,' 1896.

Ammonium-hydrogen Sulphide.—Sections taken out of the alcohol and placed in this solution instantly gave a marked dark yellow-brown reaction wherever there were green patches. This reagent is more striking in its results than the potassic ferrocyanide, and very good cover-slip preparations of the blood can be obtained, the corpuscles staining dark yellow-brown.

Hæmatoxylin.—We were led to use this reagent from knowledge of its reaction in the case of Weigert's nerve-staining method. The results are most striking. Sections placed in a watch-glass of distilled water, to which a few crystals of pure hæmatoxylin are then added, begin at once to assume a distinct blue colour in the place of the previous green; this occurs whilst the solution itself remains free from colour, and therefore whilst the quantity of hæmatoxylin dissolved must be very minute. Microscopic examination shows the corpuscles dark blue, and the vascular network beautifully differentiated. The connective tissue and gland cells and nuclei remain unstained, or occasionally show a very faint blue reaction, most marked immediately around the vessels. This reaction appears to us to be as specific for copper as Macallum showed it to be in the case of inorganic iron. Just as in the test-tube, so in the cell, a blue-black reaction is obtained not only with iron (as in the state seen in the liver cells in pernicious anæmia) but also with copper. It therefore follows that hæmatoxylin is a most sensitive test for either metal, and that consequently in the outset it is necessary to determine whether copper or iron is present exclusively in the cells, and to which of these elements the reaction is due.

Iron is found in the ash of the oyster, and the green coloration of the Marennes oysters has been attributed to it by Carazzi and others. In the case of the green oysters which we have examined, Dr. Kohn found, in addition to the copper, traces of iron—the iron was, however, far below the copper in quantity. In the detailed and valuable paper of Macallum, previously referred to, a series of histo-chemical reactions are described in order to demonstrate the presence of iron in cells, and he with others distinguishes two forms, organic and inorganic. The latter, like, we presume, the iron in the liver cells in pernicious anæmia,[*] gives an immediate reaction with potassic ferrocyanide and dilute hydrochloric acid, and as Macallum has shown,[†] a dark blue with pure hæmatoxylin. But the organic iron behaves differently, giving, according to Macallum, a yellow colour with hæmatoxylin, and requiring previous treatment with dilute nitric, sulphuric, or hydrochloric acids in alcohol before a Prussian blue reaction is obtained with acidulated potassic ferrocyanide, or prolonged

[*] We have obtained an immediate blue reaction with hæmatoxylin in the liver in five cases of pernicious anæmia.

[†] 'Report British Association,' Liverpool, 1896, p. 973.

treatment with ammonium sulphide before any dark coloration is obtained with that reagent. Now it will be observed that all our reactions were *immediate*, taking place directly on the addition of ammonium sulphide, or potassic ferrocyanide alone, or aided by a trace of acid. The copper was therefore present in a condition analogous to the inorganic iron, or at least so loosely combined with the cell protoplasm as to be readily discharged, but in none of these cases did we get any indication of inorganic iron, except in the case of the *contents* of the alimentary tract of the oyster. When the sections were treated with 3 per cent. nitric acid in alcohol for half an hour the green colour disappeared, and then neither the copper reaction nor the striking reactions with ammonium sulphide and hæmatoxylin took place. Subsequent treatment of these sections with acidulated potassic ferrocyanide, and again washing in dilute nitric or hydrochloric acid, yielded a general and very faint Prussian blue reaction, in which the nuclei of the gland cells were more markedly blue than the leucocytes. If the method is reliable it shows that traces of iron are present in the cells in addition to the copper, but it is the organic iron. Some oysters gave this Prussian blue reaction more markedly than others; this was the case with some Falmouth "natives."

Hæmocyanin containing copper has been shown to be an important constituent of the blood in many of the invertebrata, taking the place of hæmoglobin. We have examined the blood of very many oysters, and only in two instances, and these in green oysters, have we thought that the plasma became very faintly blue on exposure to oxygen, whilst, as previously indicated, qualitative tests either failed to give any indication of copper or, at most, only a very faint reaction, and even in these cases the reaction appeared confined to the leucocytes which were present in the plasma. The ash, however, of the white oyster yields about 0·006 grain of copper, and it is probable that minute traces are present in the plasma as hæmocyanin. The cause, then, of the presence of the copper in such abundance in the green leucocytes is very obscure. The quantity of copper in the green leucocytes themselves varies, as our histochemical reactions demonstrated; some corpuscles could be found which were conspicuous by their red reactions on the addition of potassic ferrocyanide, whilst others in the same preparations only gave very faint indications, and occasionally a cell could be seen which gave a marked Prussian blue reaction instead of the red.

Conclusions.

Our results demonstrated the presence of copper in comparatively large quantity in the green leucocytes, chiefly in the American oyster, but also in the "natives" from Falmouth and other localities.

We have shown that the colour was in proportion to the amount
of copper present, and that the colourless leucocytes contained
only traces of that metal. The deposition of the copper in this
large quantity appears to us to represent a degenerative reaction;
it was accompanied by a most striking increase of leucocytes,
which tended to distend the vessels and to collect in clumps,
phenomena which are abnormal in our experience in the oyster.
The presence of the copper in the leucocytes in these cases might
be compared to that of the iron which is met with in some of the
leucocytes in cases of old hæmorrhages, pernicious anæmia, or in
other cases where iron is set free. We are not prepared to state
whether copper in the food can bring about the condition, but cer-
tainly we have abundant evidence to show that it can occur where
no copper mines or other evident sources of copper are present.

We are inclined to suggest that the increase of copper may be due
to a disturbed metabolism, whereby the normal copper of the
hæmocyanin, which is probably passing through the body in
minute amounts, ceases to be removed, and so becomes stored up in
certain cells.

Our results also show that hæmatoxylin is a most valuable
reagent, not only as Macallum has shown in the case of iron, but also
in that of copper, and that care must be taken to distinguish between
the two reactions; and this must be especially the case in those
invertebrata where copper plays an important *rôle* in the physiology
of the blood.

"Stress and other Effects produced in Resin and in a Viscid
    Compound of Resin and Oil by Electrification." By J. W.
    SWAN, F.R.S. Received May 17,—Read June 17, 1897.

(PLATES 1—4.)

While making an experiment with the object of finding the degree
of resistance to puncture offered by paper coated with a soft com-
pound of resin and oil, when placed between the secondary terminals
of an induction coil, the tension being regulated by a spark-gap in a
parallel branch of the circuit, observed that on the passage of a
spark at the spark-gap, while no spark passed between the paper-
separated terminals, a sudden roughening or puckering of the
previously smooth surface of the coating took place.

A number of experiments were made with the object of ascertain-
ing the nature of the action which produced this effect, and these led
to further experiments and to results which, though closely related
to well-known phenomena, possess features of novelty and interest.

It was found that clear Bordeaux resin in a viscid state (viscidity being brought about either by heat or by the addition of resin oil) is responsive to the mechanical stress consequent on electrification by non-luminous discharges; and if it is so acted upon while in the solid state, and afterwards superficially softened by heat, there results a new kind of electric discharge figure, analogous to the dust figures of Lichtenberg and Lord Armstrong, but showing some remarkable peculiarities which throw additional light on the mechanism of air-conveyed electrical discharges, and on the location and nature of the stresses imparted to the dielectric. I ascertained that a smooth surface of resin is retentive of an electric charge to an extraordinary degree, that after more than two months the lines of an electric discharge figure, as developed by heat, and as further developed by the accidental attraction of atmospheric dust to the electrified parts of the surface, were still attractive of dust in a discriminative manner, no change being observable upon re-dusting either in the arrangement or definition of the lines of electrification as originally developed.

The apparatus employed consisted of an induction coil or a Wimshurst machine, and a supporting stand, the rod of which carried two clips and a stage, the supporting part of the stage being made of strips of thick plate-glass, and the rest of wood. The clips held conducting wires, which passed through bent glass tubes, and went to the secondary terminals of the induction coil, or to the conductors of the Wimshurst machine; the discharging arms in either case constituted an adjustable spark-gap in parallel with the wires ending above and below the stage. The stage terminals were balls, discs, or points; the pairs employed in different experiments varied in size and form, and the pair used together were sometimes dissimilar. The resin was the colour of amber; in some of the experiments it was used in a solid state, but fused to the form required for experiment; in other experiments it was softened to semi-liquidity by the addition of 20 per cent. or more of resin oil, the mixture being made by fusion together of the resin and oil. The compound with 20 per cent. of oil has the consistency of treacle at a temperature of 20° C.; at 12° C. it is nearly solid, yet plastic enough to yield to the mechanical stress-action generated by the projection upon its surface of an electric discharge of the kind employed in the experiments. At the higher temperature the viscid liquid is well suited for showing the great disturbance produced by repeated discharges, and when at the lower temperature it is convenient for observing the more persistent forms of the figures produced on the surface by single discharges under various conditions. When it was required that the stress figures should be permanent, resin either alone or with not more than 2 or 3 per cent. of resin oil was used.

The dielectric was either contained in glass basins or spread as a coating of 0·5 to 1 mm. thick upon glass or mica plates, and in a few cases on copper plates. Also plates consisting wholly of resin were in some instances used.

The effect of a spark passing at the spark-gap, when one of the stage terminals is suspended over, and at a certain distance from the viscid resin and oil mixture contained in a basin, the other being in contact with a metallic disc under it, is to produce an evanescent figure on the surface.

The character of the figure depends on :—

1. Whether the terminal over the dielectric surface is positive or negative.
2. The form and size of the + and − terminals.
3. The distance of the upper terminal from the surface of the dielectric.
4. The potential and character of the spark at the spark-gap.

Typical Effects.—The most regular and characteristic stress figures are obtained when the spark-gap is adjusted so as to prevent the passage of a spark or visible brush through or over the dielectric, but allow a non-luminous discharge to take place of only slightly less strength than would be necessary to produce a brush discharge visible in the dark. A typical effect is obtained when the spark-gap is 25 mm., and the positive branch from it terminates in a brass ball of 8 mm. diameter hanging centrally over, and 4 mm. from the surface of, the dielectric (80 per cent. resin and 20 per cent. resin oil at 20° C.) contained in a glass basin 150 mm. diameter and 15 mm. deep, the negative wire being brought to a disc of metal 100 mm. diameter under the basin, or to a disc of tinfoil attached to the underside. On breaking the primary circuit by means of a mercury break with a trigger action (the spark-gap having been momentarily short-circuited while the primary circuit was closed), and the consequent passing of a single spark at the spark-gap—no visible discharge occurring between the ball and the dielectric—there suddenly breaks out on the surface of the viscous liquid a star-shaped figure formed of deeply furrowed, closely clustered, outward-branching rays, extending from a circular frill near the centre to the margin of the liquid. The figure gradually dies down, and on the surface becoming smooth it can, with slight variations, be reproduced again and again by repeated breaks of the primary circuit.

If the commutator is reversed (the spark-gap being momentarily closed while the primary circuit is re-made), then on breaking the primary circuit as before, a figure characteristic of the negative convective discharge is produced. This figure is much smaller and weaker than the positive one; most frequently it consists of a

circular, or nearly circular, band or ring, more or less indented in outline, enclosing leaf-like rays which tend towards the centre. These are relatively broader and less branching than the rays of the positive figure, and they are characterised by having their outlines in relief, while the rays of the positive figure are sunk below the plane of the surface. When the electrification is strong, the ring enclosing the rays stands up as a frill in considerable relief.

Effect of the Form of the Terminal.—The character, both of the positive and negative figures, is greatly affected by the form of the discharging electrodes. When the upper terminal is a metallic disc of 25 mm. diameter, hanging in a plane parallel to, and 2 mm. above, the dielectric surface, and the other terminal is a 50-mm. metallic disc supporting the basin, complicated, but nearly symmetrical, figures of great beauty are obtained. A metal point opposed to a metal point, or a metal point above and a small metal ball below, give smaller figures of more elementary forms, having the general characteristics of the larger figures.

Balls and points as terminals tend to produce circular figures in which the rays converge to, or diverge from, one centre. This rule applies with fewer exceptions to the + figure. The negative figure, even when produced by a discharge from a brass ball, is frequently a combination of sectors, whose centres are not far apart, and are concentric with the centre of the group. The effect of this is to produce a figure of nearly circular outline broken by more or less regular indentations.

If the ordinary vibrating contact-breaker be employed instead of the trigger-break used in the foregoing experiments, the rest of the arrangement remaining as described, larger and more complicated effects are obtained. When the ball above the liquid is positive, the resin and oil being at a temperature of 20° C., on breaking the primary circuit the first effect of the make-and-break is the production of the characteristic star with arborescent rays; the repetitions of the impact which instantly follow indent the lines of the figure more and more deeply, and result in the effacement of the more regular figure, and the development of a large and turbulent movement of the liquid, tending to its division into two masses: a central mound with a flat or concave top and a concentric ring. At first the two masses are joined by radial ridges, but these gradually thin, and (if the discharges at the spark-gap are continued) eventually break down and leave the central mound and the embracing ring completely separated. During the progress of the action the ring portion is driven outwards, and when the limit of outward movement is reached, there is a subsidence of the more violent agitation, the outer ring becomes somewhat smoother and flows inwards; this is followed by a recurrence of the repellent action, and a repetition of the last

phase of the phenomena described. Figs. 1 and 2 represent in profile the appearance at the middle and final stage of the action.

FIG. 1.—*Positive.* Nearly Maximum Effect.

FIG. 2.—*Positive.* Maximum Effect.

If a metal ring (of 90 mm. diameter) is substituted for the disc, similar but more sharply defined effects are obtained. When the arrangement described is varied by making the ball over the basin negative, instead of positive, on breaking the primary circuit there is less displacement of the viscid material consequent on repeated interruptions of the circuit. To obtain a characteristic effect of repeated negative discharges, the spark-gap should be widened to 50 mm., and to prevent sparking over the edge of the basin, it should be at least 150 mm. diameter. There is then formed, immediately under the ball, a concavity the counterpart in size and curvature of the ball, and outside this there is the general figure, somewhat faint and tremulous, which seems to be a complication of the positive and negative figures, the characteristic positive figure encircling the more distinctly negative portion. Fig. 3 is a profile view of the negative displacement. The extent of the effects described is considerably modified by the degree of viscosity of the liquid, and this can be controlled by temperature.

FIG. 3.—*Negative.* Maximum Effect.

Corresponding but modified phenomena are produced by means of the Wimshurst induction machine. When, for example, an 8 mm. metal ball connected to the + arm of the discharger hangs 15 mm. from the surface of the resin and oil mixture of the consistency of treacle contained in a large basin, and the — arm of the discharger is connected to a metal disc of 100 mm. diameter under the basin, the discharger balls being 75 mm. apart, the result of continuously working the machine is the production of a turbulent motion, attended by the

formation of vortices into which the upper stratum of the liquid pours downward, carrying adherent air with it, while an equal and opposite movement takes place from below, producing worm-like eruptions of spirally twisting or wriggling jets of liquid at the surface. If the margin of the dish beyond the surface of the liquid is thinly smeared with the viscid liquid, the film breaks up into dew-like beads.

With a metal ball of 150 mm. diameter, 6 mm. above the surface of the liquid and a proportionately larger disc basin and depth of liquid, acted upon by a machine of moderate power, the spark-gap being adjusted so as to prevent sparks passing from the ball to the liquid, and the tension such as to keep up a strength of electrification of the surface only slightly less than that which would cause disruptive discharges to pass, a column of liquid rises and connects the overhanging ball and the surface of the liquid in a manner strongly suggestive of water-spout phenomena. The ascent of the column of liquid is followed by the descent of numerous thin surrounding streams, and these keep up a regular system of upward and downward circulation.

Fixation of Characteristic Effects.—In order to fix the various forms produced in the viscous mixture of oil and resin, an experiment was made with nearly pure resin, rendered plastic by heat, and cooled to solidity while under the action of electric discharges. It was found to be difficult to carry this out in a satisfactory manner, but it suggested a reversal of the procedure, viz., the electrification of a surface of resin in a solid state, and the subsequent development of the stress effect by rendering the surface superficially plastic by heat. This mode of operation resulted in the production of permanent relief and intaglio figures, corresponding to Lichtenberg's dust figures, to Brown's photographs,* and to the dust figures and photographs more recently described by Lord Armstrong.†

The preparation of the resin surface for the production of the permanent figures requires care. The method I employed is as follows :—A thin glass basin was filled with a mixture of resin and 5 per cent. resin oil, the oil being added to lessen the tendency of the resin to fracture on sudden change of temperature. The resin, melted in a metal pan, was poured through a filter of muslin into the basin, while embedded in small shot and raised to the fusion temperature of resin. On slowly cooling, the basin being meanwhile covered with a plate of glass or an inverted basin, the resin solidifies with a smooth bright surface. The resin-filled basin was partly covered, on the bottom, by a disc of tinfoil, and was placed centrally

* 'Phil. Mag.,' vol. 26, p. 502.

† 'Electric Movements in Air and Water, with Theoretical Inferences,' by Lord Armstrong, C.B., F.R.S. London : Smith, Elder and Co., 1897.

on a metal disc in contact with one of the wires from the spark-gap
of the induction coil, the other wire, ending in a point, disc, or ball,
overhanging the surface at the distance of a few millimetres as in the
experiments with the viscous material described.   On the production
of a single spark at the spark-gap by means of the trigger-action
mercury contact-breaker, a charge is conveyed to the resin.   The
peculiar distribution of this charge, and that it is attended by strong
and enduring mechanical stress, can be made manifest either immedi-
ately, or many hours afterwards, by slightly warming the surface of
the resin.   The result is a deeply impressed figure, having the same
character as the figure produced on the viscid dielectric.   These
solid figures, if carefully developed, show much fine detail; unfortu-
nately, this is not capable of complete illustration by photographs.
Difference of depth in the grooves is not fully indicated, neither is
there represented adequately a peculiar burring of the margin of the
grooves, especially in the negative figures, their edges rising slightly
above the plane surface, as though the resin had been finely carved.

Persistency of the Stresses.—The persistency and fixity of the
electrification of the resin surface, determining the form and
character of the eventual figure, are very remarkable.   If the
development of the effect of the charge is delayed for twenty-four
hours there is but little difference in the result from that which
follows immediate development.

Dust Figures and Stress Figures combined.—It was found that the
heat-developed figures attracted dust from the atmosphere, and thus
formed, accidentally, a combination of a stress figure and a dust
figure.   The attracted dust gave clear indications of electrification
beyond the limit of the stress figure, and brought out features of
detail which helped to explain the nature of the electrification.
Analysis of the character of the figures in this respect is still further
helped by combining a modification of the dust process of Lichtenberg
with the stress effect described in this paper.   The modification
referred to consists in allowing the dust—the mixture of red lead
and sulphur proposed by Lichtenberg—to be *drawn up* to the elec-
trified surface from a cloud of dust.   This assists in the selective
appropriation of the two substances, giving a redder colour on the
negatively electrified portions of the figure, and a yellower colour on
the positive portions ; and showing what the stress figure alone does
not show with equal clearness, how inseparable are the + and −
actions.   The best effect is obtained by applying the dust process
before development by heat.   Figs. 4 and 5 (Plate 1) represent
characteristic forms obtained by positive and negative discharges
with a metal ball electrode above the surface and a metal disc below,
fig. 4 being + and fig. 5 −.   Figs 6 and 7 are corresponding figures
obtained with discs above and below, fig. 6 being + and fig. 7 −.
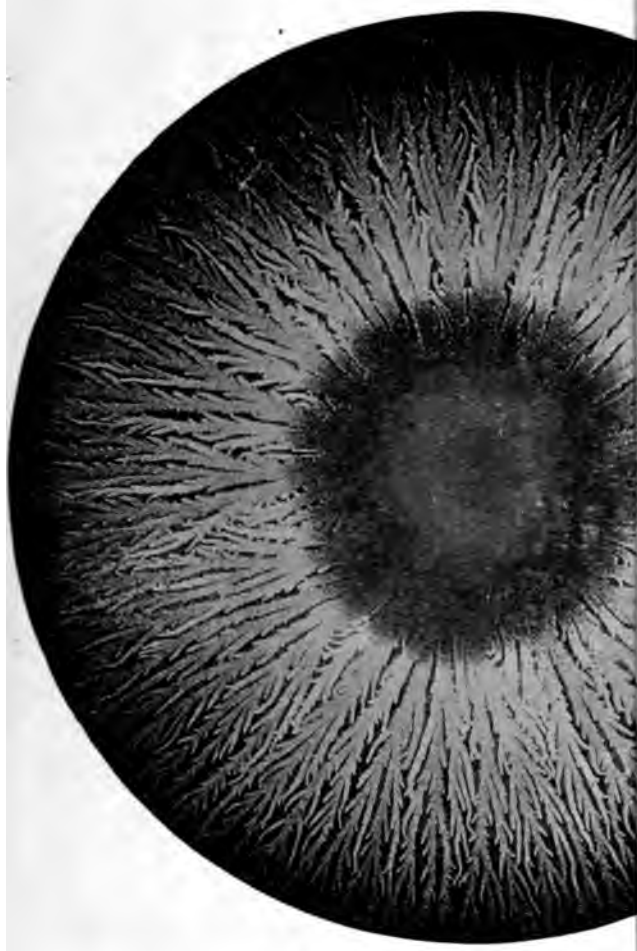
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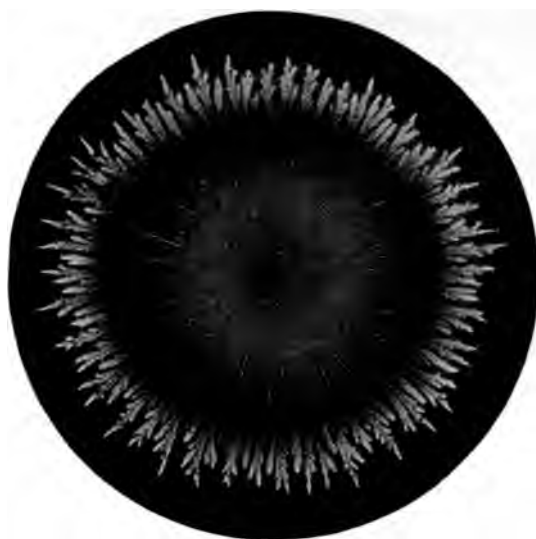
Fig. 5.

Fig. 6.
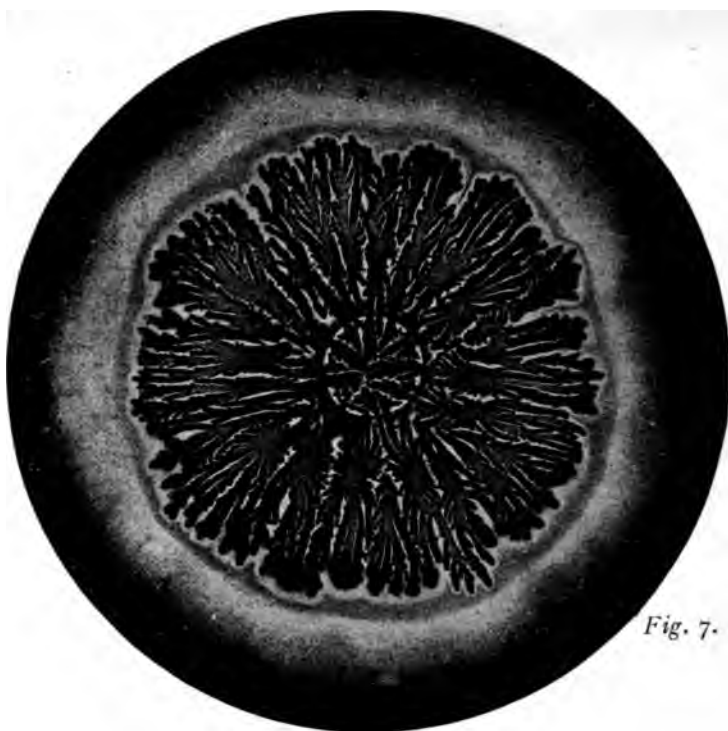
Fig. 7.

Fig. 8.
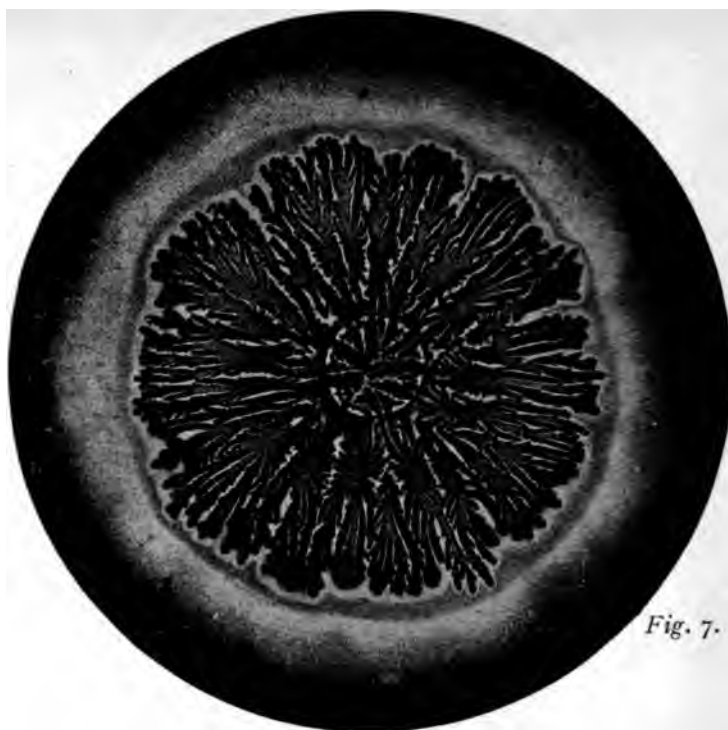
Fig. 7.

Fig. 8.

Fig. 9.

Fig 10.

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Figs. 8 and 9 are additional examples of negative discharge figures with larger disc electrodes.

An excellent dust figure, in which the result of + electrification is strongly developed, is obtained by suspending, face downwards, an electrified resin surface in a very thin fume-cloud produced by burning magnesium ribbon. The fume should be enclosed in a box, or under a glass shade, and an hour should elapse for the coarser particles to subside before the introduction of the electrified surface. Sulphur in a state of sublimation can also be used in the same way with good effect, especially for very small areas of electrification, where microscopically fine development is required. On the whole, however, I have found nothing better than red lead and sulphur ground separately to very fine powder, and used very dry in a dusting box, the electrified surface always being downward when exposed to it.

With the object of finding the degree and kind of interaction between the positive and negative electrification produced on opposite sides of a solid dielectric, interposed in the path of a single discharge, the following experiment was made :—A thin plate of glass was coated on both sides by dipping in melted resin, this was electrified by bringing the secondary terminals of the induction coil, arranged as in the experiments already described, to opposite sides of the plate. The terminals were brass balls 8 mm. diameter, placed in a vertical line, the + above, the — below the plate in a horizontal position ; the + ball was 1 mm. distant from the upper surface of the plate, and the — ball was in contact with the under surface. Under these conditions when an 8-mm. spark passed at the spark-gap, reciprocal figures of a very interesting character were produced, a + figure on the upper surface and a — figure on the under surface.

To enable photographs to be taken of these figures without interference, the experiment was repeated with the variation that a plate of ruby glass coated with resin on both sides was used instead of clear glass. The latent figure was first developed by means of the red lead and sulphur cloud, and afterwards the stress effect was brought out by heat. Fig. 10 shows the form of the figure on the + side, and fig. 11 that on the — side. When these double figures are viewed by transmitted light, it is seen that the interior — rays on one side, coincide with the inner ends of the outward streaming + rays on the opposite side.

That the depth of penetration of the charge which produces these figures is very small is shown by the almost complete discharge effected by washing the electrified surface with water.

The experiments seem to show that when an electric discharge takes place through air, its propagation is attended by a structural arrangement of the air brought under the influence of the discharge,

and that when a dielectric like resin is interposed in its path, some of the characteristics of the form into which the electrified air has been thrown are transferred to the resin surface as an electric charge, generating the stresses and other inductive effects which result in the dust and stress figures.

Experiments corresponding to those described made in an atmosphere of carbonic acid gas at normal atmospheric pressure, and in air at pressures lower than the normal, show that the character of the figure imprinted on a dielectric in receiving an electric charge through a gaseous medium is largely dependent on the density of the atmosphere conveying the charge; greater density tending to concentration of the figure and attenuation to diffuseness. With an air pressure supporting 85 mm. of mercury, the other conditions being such as would have given at normal pressure a characteristic + star figure, there was diffuse electrification of the resin surface, but there were no rays.

" On the Brains of two Sub-Fossil Malagasy Lemuroids." By C. I. FORSYTH MAJOR. Communicated by HENRY WOODWARD, LL.D., F.R.S., V.P.G.S. Received April 6,—Read June 3, 1897.

(PLATE 5.)

The casts here described and figured have been moulded from the brain-cavities of the skulls of two sub-fossil Lemuroids from Madagascar, the descriptions of which I have already published. For comparison with the brains of living Lemuroids the figures published by P. Gervais* are the best adapted for the *present* purpose, since they, too, are drawn from moulds of the brain cavity, and give on one plate a good general idea of the variations of the Lemur brain.

1. *Globilemur Flacourti*, Maj.

The larger of the two casts was taken from the skull briefly described by me at the meeting of the Zoological Society of London, June 20, 1893.†

In its general contours, as viewed from above (fig. 1), the brain of this form, for which I now propose the name of *Globilemur Flacourti* (*g. n. et sp. n.*), approaches most to that of the smallest members of the family (Lemuridæ), viz., *Microcebus*,‡ both being remarkably broad

* Paul Gervais, " Mémoire sur les formes cérébrales propres à l'ordre des Lémurs, accompagné de remarques sur la classification de ces animaux," ' Journal de Zoologie,' vol. 1, 1872, pp. 5—27, Pl. 2.

† ' Zool. Soc. Proc.,' 1893, pp. 532—535.

‡ P. Gervais, *loc. cit.*, fig. 7, Pl. 2.

in their posterior moiety and suddenly attenuated anteriorly. Apart from the Sylvian fissure, the brain surface of *Microcebus* is perfectly smooth, whilst the cast of the fossil shows a greater complication than in any other known Lemurid. This is in accordance with what might have been anticipated, *Globilemur* being larger than any living Lemurid, and, as Broca states : " *Un cerveau qui grandit doit se plisser sous peine de déchoir** " ; this, in my opinion in fact, means that for economy of space plication is resorted to as a means of increasing the surface.

In the arrangement of its convolutions (fig. 2), the fossil departs likewise from what is known of Lemurid brains, and approaches rather more to what is presented by some of the larger *Cebidæ* and *Cercopithecidæ*. In Lemurids the fissures and the corresponding convolutions show a tendency towards a longitudinal arrangement, quite different from the more radiating direction exhibited by the fossil. Its Sylvian fissure (*s.f.*), on the other hand, corresponds in its more vertical direction to what we find in Lemurids, and in this respect departs more from the Old and New World monkeys, though less from the former than from the latter. The character mentioned is in relation with the development of the occipital lobe, the Sylvian fissure being always more horizontally directed in those brains in which the occipital lobe is well developed and in which, as a consequence, the cerebellum is covered. In fact, in *Globilemur*, the cerebellum is much less overlapped than in the monkeys.

In the lesser development of the frontal lobes we find a further agreement with Lemurids as compared with monkeys, and equally so in the more macrosmatic character of the brain of *Globilemur*, as revealed by its voluminous olfactory lobes.

I shall not enter into farther particulars as it is never safe to attempt to make out the exact homologies of the fissures in a cast of the brain cavity. Moreover, in this case, I find that the two sides of the hemispheres do not agree in every respect, owing partly to the incomplete condition of the skull and partly to the difficulties encountered by the artist in the moulding.

II. *Megaladapis madagascariensis*, Maj.

The second cast, from the brain-cavity of *Megaladapis madagascariensis*, is in many respects the very opposite of *Globilemur*. First, as to size,—from the dimensions of the respective skulls, the size of the first named animal (*Megaladapis*) may be approximately calculated as double that of the last (*Globilemur*), whilst in bulk the brain

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* Paul Broca, "Anatomie comparée des Circonvolutions Cérébrales. Le grand lobe limbique et la scissure limbique dans la série des Mammifères," ' Revue d'Anthropologie,' II, vol. 1, 1878, p. 413.
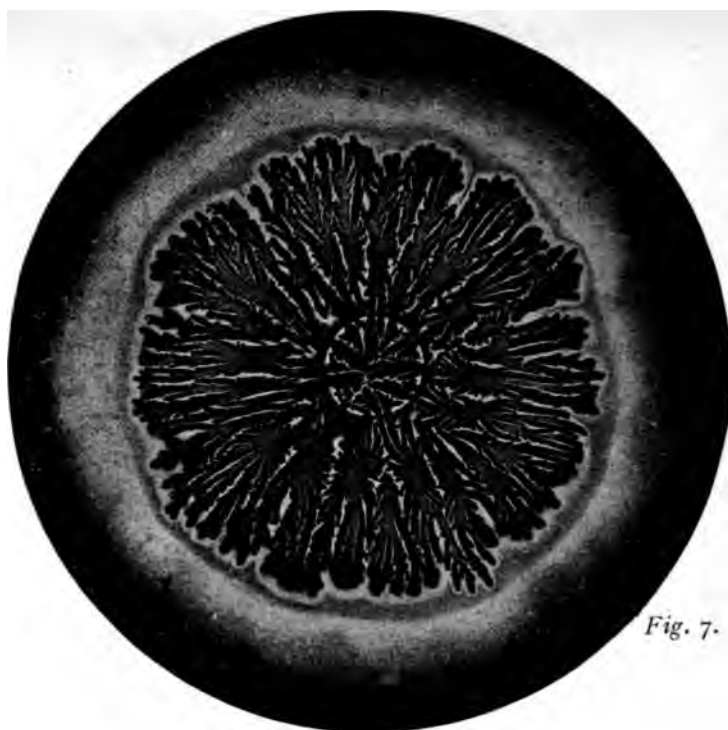
Fig. 7.

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occluded, and the heat evolved represented the true heat of occlusion of this quantity of oxygen.

Indirectly, the same value was obtained by charging the platinum black up fully and alternately with hydrogen and oxygen, and finally with oxygen. The amount of oxygen really occluded in the last charge, and independent of that which had gone to form water, was then found by exhausting *in vacuo* at a red heat. The difference between this quantity and the total amount of oxygen used is a measure of the oxygen which formed water with twice as much hydrogen by volume. Knowing these quantities, the total heat evolved, the heat of formation of water, and the heat absorbed on the removal of hydrogen, we have all the data for calculating the heat of occlusion of oxygen.

In a similar way the amount of heat *absorbed* per gram of oxygen *removed* was calculated from the data obtained during the penultimate charge.

The mean value for the heat of occlusion of oxygen, from the direct and indirect measurements, which did not differ much from each other, is $+11\cdot0$ K (1100 g-calories) per gram. This value referred to 16 grams of oxygen is $+176$ K, which is almost identical with Thomsen's measurement of the heat of formation of platinous hydrate $Pt(OH)_2$, viz., $+179$ K.

This agreement suggests the possibility that the two phenomena may in reality be identical, the necessary water being always present in platinum black dried *in vacuo*.

The paper concludes with some speculations on the nature of the occlusion of gases.

"On the Appearance of the Cleveite and other New Gas Lines in the Hottest Stars." By J. NORMAN LOCKYER, C.B., F.R.S. Received June 15.—Read June 17, 1897.

Introductory.

In my recent paper on "The Chemistry of the Hottest Stars," [*] I left for future discussion the spectra of those stars apparently at or near the apex of the temperature curve, for the reason that in them the lines of known gases do not show very great variations, while the enhanced lines cease to be of service as a criterion of temperature. I pointed out, however, that there were several lines, as yet of unknown origin, which are strong in some of these stars and weaker in others, and that the study of these might eventually help us in classifying such stars and arranging them in temperature

order, but that before attempting to use the unknown lines in these inquiries it was important in the first instance to discriminate, if possible, between gaseous and metallic lines. Until this point was investigated the relative behaviour of the lines of hydrogen and cleveite gases near the upper temperature limit could not be satisfactorily discussed.

The work has now been carried on a stage further, and in the present paper I propose to give the results of the inquiry into (1) the appearances of the lines of gases, both old and new, in the spectra in question, and (2) the most probable sequence of temperature in the stars under discussion.

The Spectral Lines by which the Sequence of the Hottest Stars can be determined.

In the former paper I stated the sequence of certain stars, both of increasing and decreasing temperature, as determined chiefly by the enhanced lines of iron and the lines of the cleveite gases. At the junction of the two series I provisionally grouped together Bellatrix, ζ Orionis, η Ursæ Majoris, λ Tauri, and γ Pegasi, pointing out that their spectra were not quite identical and might afterwards be separated when the criteria had been further studied.*

Further inquiry has shown that γ Pegasi may be regarded as practically identical with Bellatrix, while η Ursæ Majoris and λ Tauri differ from it chiefly in the general haziness of the lines; no attempt has been made, therefore, to separate these stars from Bellatrix. Other stars included in the present discussion were δ and ε Orionis. At the top of the ascending series of stars I placed Rigel and ζ Tauri,† and, among others, at the top of the descending series were β Persei and α Andromedæ.

The sequence of the still hotter stars can, therefore, be determined by an investigation of the varying intensities in their spectra of lines which appear; also in stars on one side or other of the temperature curve. The principal lines utilised in this inquiry are as follows:—

* 'Roy. Soc. Proc.,' vol. 61, p. 180.

† This is one of the most extraordinary spectra which has been met with in the Kensington series of photographs, as I have already pointed out (' Roy. Soc. Proc.,' vol. 61, p. 184). While the lines of hydrogen are fairly sharp and not very broad, many of the lines, especially those of the cleveite gases, are broadened almost into invisibility. On the meteoritic hypothesis this is explained by the great differences of velocity and direction of the meteoritic streams, the special broadening of the lines of the cleveite gases indicating that these gases are chiefly concerned in disturbances at high temperatures.

On account of the indistinctness of many of its lines, ζ Tauri is omitted from the present discussion.
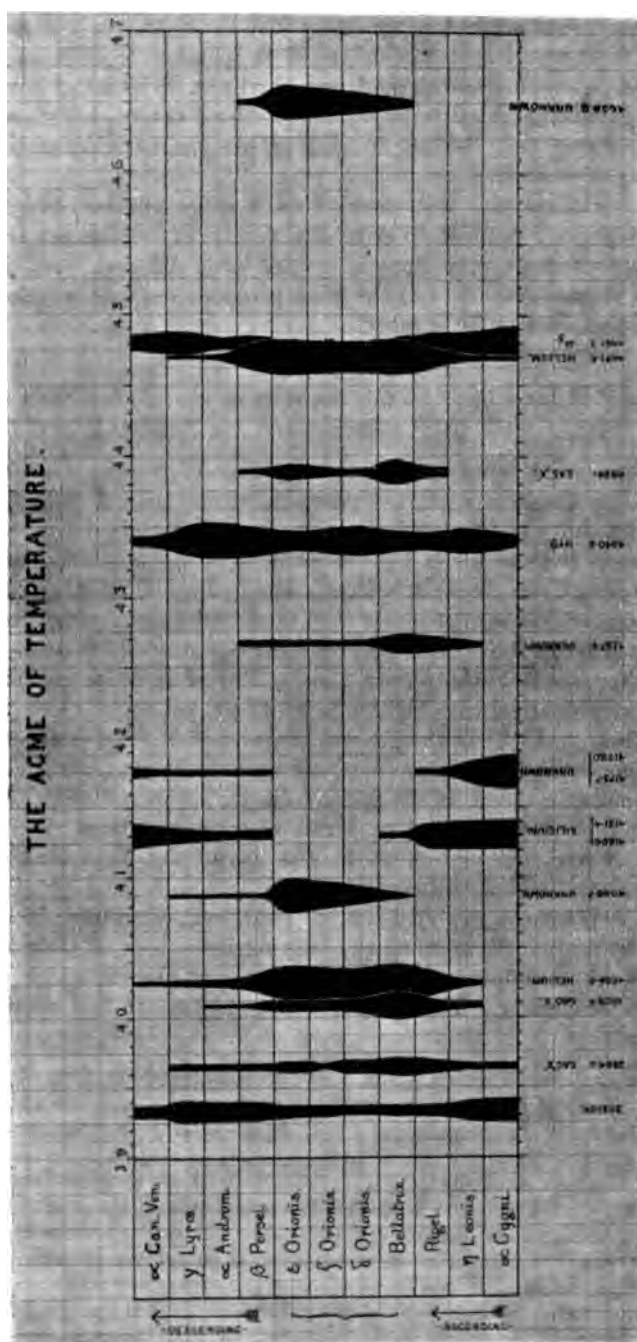
3933·8 Ca(K)	4173·2 unknown.
3964·8 Gas X	4179·0 unknown.
4009·4 ,,	4340·6 Hg.
4026·3 He	4267·6 unknown.
4088·7 unknown	4388·1 Gas X.
4128·6 Si	
4131·4 Si	

The accompanying map shows the sequence of spectra in the hottest stars deduced from the behaviour of the above lines in passing from the stars of increasing temperature to stars of decreasing temperature, and includes also some of the typical stars on both sides of the curve, namely, α Cygni, η Leonis, and Rigel, and β Persei, α Andromedæ, γ Lyræ, and α Canum Venaticorum, in the order previously determined. In each case the intensities of the various lines are indicated by their thicknesses, so that the variations in passing from star to star are plainly shown. The wave-lengths of the lines and the origins of the known lines are shown at the bottom of the map.

The map enables us to discuss the relative behaviour of each of the lines, and to notice which thin out or become more intense as the temperature changes.

It will be seen that when one set of lines becomes very faint or disappears, another makes its appearance or becomes intensified.

The map thus shows the most probable sequence of spectra among the stars near the acme of temperature as deduced from the changes of intensity of the lines given above.

The Variations of the Cleveite Gas Lines.

Comparison of the Principal Lines of Helium and Gas X.—In discussing the appearance of gas X in relation to helium, it is necessary to deal with the subordinate series in each case, as the only line of the principal series of helium (λ 3888·785) which falls in the photographic region considered coincides with a hydrogen line, and cannot therefore be compared with the line of the principal series of gas X, which does come within range. Taking the lines 4471·6 and 4026·3 as representing helium, and 4388·1 and 4009·4 as representing gas X, the comparison shows that :—

1. Gas X does not vary absolutely with helium.
2. Gas X increases its intensity at a different rate from that of helium.
3. When helium is at about a maximum so is gas X. The maximum of gas X is, however, very short lived, while that of helium extends very considerably.

These differences are shown on the map, and they fully accord with the laboratory work, which indicates that helium and gas X are to be regarded as distinct substances.

Comparison of the Lines of the Subordinate Series.—In the above investigation it has been found that, in tracing the progress of gas X through the stars of increasing and decreasing temperature in the photographic region, the relative intensities of the lines of the different series are changed from those tabulated in the laboratory. The lines of the principal series, as indicated by the line at 3964·9, are no longer the strongest, but become of secondary importance as regards intensity, whilst the first subordinate series now takes the pre-eminent position, and the second subordinate series nearly disappears altogether, being only represented very feebly near the point of highest temperature.

In the following tables, drawn up by Mr. Shackleton, will be found a statement of the relative intensities of the principal, first subordinate, and second subordinate series of gas X and helium.

Relative Intensities in Stars of increasing Temperature of the Lines in the principal and subordinate Series of Gas X.

Star.	Principal series (λ 3964·9).	1st subordinate series (λ 4385·1).	2nd subordinate series (4437·7).
Bellatrix............	5	7	1
Rigel.............	4	3	—
η Leonis......	2	? trace	—
α Cygni	2	1	—
γ Cygni	—	—	—
α Orionis...........	—	—	—

Relative Intensities in Stars of increasing Temperature of the Lines in the 1st and 2nd subordinate Series of Helium.

Star.	1st subordinate series (λ 4471·6).	2nd subordinate series (λ 4121·0).
Bellatrix............	10	4
Rigel	6	2—3
η Leonis............	2	1
α Cygni	1	1—2
γ Cygni	—	—
α Orionis...........	—	—

Relative Intensities in Stars of decreasing Temperature of the Lines
in the principal and subordinate Series of Gas X.

Star.	Principal series (λ 3964·9).	1st subordinate series (λ 4388·1).	2nd subordinate series (λ 4437·7).
Bellatrix.............	5	7	1
β Persei	2	3	—
γ Lyræ	2	1	—
Sirius	—	—	—
Castor..............	—	—	—
Procyon	—	—	—
Arcturus	—	—	—

Relative Intensities in Stars of decreasing Temperature of the Lines
in the 1st and 2nd subordinate Series of Helium.

Star.	1st subordinate series (λ 4471·6).	2nd subordinate series (4121·0).
Bellatrix...........	10	4
β Persei	3	2—1
γ Lyræ	1	1
Sirius	—	—
Castor..............	—	—
Procyon	—	—
Arcturus	—	—

The above detailed investigations show that while helium and
gas X behave differently as regards their appearance in stars, the
constituent series of each, so far as we can at present study their
behaviour, do not exhibit any remarkable differences. Thus in the
stars of increasing temperature there is a steady increase of intensity
of the three series of lines of gas X, and of the two series of helium
lines, while in the case of cooling stars there is a decrease in the
intensity of each series.

In the case of gas X it will be seen that the principal series is not
intensified to the same extent as the first subordinate series in passing
from Rigel to Bellatrix, and this seems to suggest that the molecules
corresponding to the principal series do not survive so high a tem-
perature as those which produce the lines of the first subordinate
series.

There is, however, no sufficient reason for regarding the three
series of gas X or of helium as representing separate constituents of

those gases, and for the present, at all events, each of the three series of helium or gas X may be taken to represent the vibrations of molecules of the same gas but of different complexities.

The differences in the stellar behaviour of helium and gas X have been confirmed by reference to the researches of Professor Vogel[*] and Professor Pickering.[†]

I suggest that the time has come to give gas X a definite name. It will be remembered that I pointed out in May, 1895,[‡] that helium was only one constituent of the gas discovered by Professor Ramsay, which he imagined to consist of helium alone, and that there was spectroscopic evidence suggesting at least one other new element associated with helium.

Afterwards, in September, 1895, Professors Runge and Paschen came to the same conclusion,[§] but their work still left indeterminate the number of elemental gases in the mixture.

In the many comparisons of the lines I had to make in my investigations I soon found the inconvenience of not having a name for the gas which gave 667, 501, and other lines, and I called it gas X for laboratory use. When, therefore, Professors Runge and Paschen, who had endorsed my results, and had extended them, called upon me, I thought it right to suggest to them that, sinking all questions of priority, we should all three combine in suggesting a name for this gas, the elemental character of which we had demonstrated.

This offer they declined,[||] and so far as I was concerned the matter dropped.

In the meantime Dr. Stoney has suggested the name " parhelium." But seeing that this word is already in use in another connection for a " mock-sun," its acceptance is, I think, impossible. I propose, therefore, the word " asterium," since it is in the stars that the behaviour of the new element has been best studied, and its appearance furnishes valuable evidence as to their chemistry.

The probable Existence of other New Gases in the Hottest Stars.

Discrimination between Gaseous and Metallic Lines.—The lines of helium, asterium, and hydrogen in the hottest stars are accompanied, as I have stated, by others which may either represent gases of a similar character, or metals at very high temperatures. It becomes important to consider the means at our command for distinguishing between gaseous and the metallic lines.

    * 'Astrophysical Journal,' 1895, vol. 2, p. 333.
    † 'Annals of the Harvard College Observatory,' vol. 28, Part I.
    ‡ 'Roy. Soc. Proc.,' vol. 58, p. 194.
    § 'Nature,' vol. 52, p. 321.
    || 'Science Progress,' June, 1896, p. 273.

One possible method is this. In the nebulæ are found the lines of hydrogen, helium, and asterium associated with other bright lines of unknown origins; it is fair to assume that if other similar gases exist in the nebulæ, the other bright lines should belong to them. In the nebulæ all these probably exist at low temperatures, since no indication of the enhanced lines of Fe, Mg, Mn, Ti, &c., have been detected in the spectra of nebulæ, and, on this ground we are driven to give up the old arguments in favour of the high temperature of the nebulæ, which depended for their validity upon the presence of "chomospheric" lines in the spectrum. The discovery of terrestrial helium has enabled its behaviour, when rendered luminous, to be studied, and we now know that its presence in a spectrum is no proof of a very high temperature.

Further, of all the lines other than hydrogen, helium, and asterium, so far discovered in the nebulæ, it would appear that only a few, if any, are certainly produced by metallic vapours.*

If, then, their origins be gaseous, as opposed to metallic, we should expect to find these lines in the spectrum of those stars in which the absorption of hydrogen and the cleveite gases which are associated with them in the nebulæ is strong. At present, this method of separating the gaseous from the metallic lines in the hotter stars cannot be finally applied, for the reason that the wave-lengths of many of the nebular lines are not sufficiently accurate for the object in view. But there is little doubt that it will furnish a valuable criterion when photographs with larger dispersion become available.

Another possible method, however, is open to us. In α Cygni, where the enhanced metallic lines are so strongly developed, the helium lines appear very feebly, and it is only in stars at still higher temperatures that helium is strongly represented. Hence, if there are other gases which behave like helium, in stars as well as nebulæ, they would be intensified in passing from α Cygni through successively hotter stars, while the enhanced metallic lines become feebler. Some of the principal lines which become thus intensified in passing to the hottest stars are indicated in the following table.

Five of the lines given in the table approximately coincide with enhanced lines, two with lines of cadmium and three with lines of sulphur, but since in the spectrum of the former substance there are fifteen enhanced lines in the same region, and in the latter twenty-nine, the coincidences may for the present be regarded as accidental.

It seems highly probable therefore that the lines recorded in the table represent gases which have yet to be discovered, and that the

* 'Phil. Trans.,' A, 1895, vol. 186, p. 76.

Lines other than Hydrogen and Cleveite Gases which make their appearance only at Temperatures higher than *a* Cygni, or become intensified at higher Temperatures.

Wave-length.	*a* Cygni.	Rigel.	Bellatrix.	ζ Orionis.
3919 ·2	—	—	3	—
3994 ·7	—	2	5	3
4010 ·6	—	—	3	—
4069 ·7	—	—	3	3
4071 ·7	—	—	3	—
4075 ·7	—	—	3	3
4088 ·7	—	—	—	8
4094 ·7	—	—	—	3
4104 ·8	—	—	3	—
4114 ·8	—	—	—	7
4172 ·6	—	—	2	—
4253 ·6	—	—	3	—
4267 ·6	—	3	7	2
4314 ·6	—	—	—	2
4345 ·6	—	—	3	—
4415 ·2	—	—	3	2
4541 ·8	—	—	—	2
4566 ·8	—	—	3	—
4574 ·8	—	—	3	—
4613 ·8	—	—	3	—
4643 ·8	—	—	3	—
4650 ·9	—	—	3	10

intensification of lines in the hottest stars in passing from *α* Cygni is a trustworthy criterion for gaseous lines.

If there are other gases which, like hydrogen, give indications of their presence at the temperature of *α* Cygni, or lower, the lines in the spectrum do not, like those of hydrogen, become more intense with increased stellar temperature. In such cases it does not seem likely that anything short of the actual discovery of terrestrial sources of the gases can help us to differentiate the lines belonging to them in stellar spectra from those due to metallic substances.

Attempts to trace Terrestrial Sources of the New Gases.—In a series of papers communicated to the Royal Society I have given an account of the attempts which I have made to find new gases by experiments upon minerals similar to those adopted for the extraction of helium and the associated gases from cleveite. In the last paper of that series I summarised the results which had been obtained, indicating that lines occurring in the spectra of gases from minerals for which no known origin could be assigned were represented in the spectra of some of the hotter stars.[*]

From this I extract the following list of the lines thus found to

* 'Roy. Soc. Proc.,' vol. 60, p. 133.

have probable counterparts in the hotter stars, and show the intensities in different stars. Lines which have since been found to correspond with enhanced lines in the spectra of any of the substances so far examined are omitted.

Stellar Lines probably coincident with Lines in the Spectra of Gases from various Minerals.

Wave-length.	α Cygni. Max. = 10.	Rigel. Max. = 10.	Bellatrix. Max. = 10.	δ Orionis. Max. = 10.
3929·4	—	—	—	1
3961 6	3	—	—	—
4002·9	5	—	—	—
4069·7	—	—	3	3
4072·2	1	—	3	—
4114·6	—	—	—	7
4309·4	1	—	—	—
4338·0	5	—	—	—

The lines thus observed in the spectra of mineral gases may be divided into two groups, the first comprising those which are strongest in α Cygni and thin out at higher temperatures, and the second those which are either absent from α Cygni or become stronger as the temperature of α Cygni is exceeded. The lines of the first group behave like the enhanced lines of the metals, and, unless the gases can be isolated, it is impossible to say whether the corresponding stellar lines are produced by gases or the coincidences are merely accidental.

In the case of the second group of lines, however, the probability that the mineral gases which give them really represent new lines is much greater, since, like the lines of helium, they are most intense in the stars which we have every reason to believe to be the hottest.

Attempts to Trace Series of Lines.—A minute examination of Bellatrix has been made by Dr. W. J. S. Lockyer with a view of inquiring whether some of the many still unknown lines might possibly form series like those of helium and asterium.

With this object the lines in the spectrum were carefully plotted, special attention being given to the intensities of the individual lines. An examination of the residual spectrum formed by omitting all those lines the origins of which were known, showed that possibly two further series were present, but a better photograph of the star spectrum is required to settle the matter definitely.

The residual lines in the spectrum of Bellatrix in the existing photographs after hydrogen, helium, and asterium have been with-

drawn, amount to upwards of fifty, and there can be no doubt that
some of them represent gases not yet discovered on the earth. It
may also be stated that these gases behave differently as regards
their range of visibility through stars of varying temperatures.
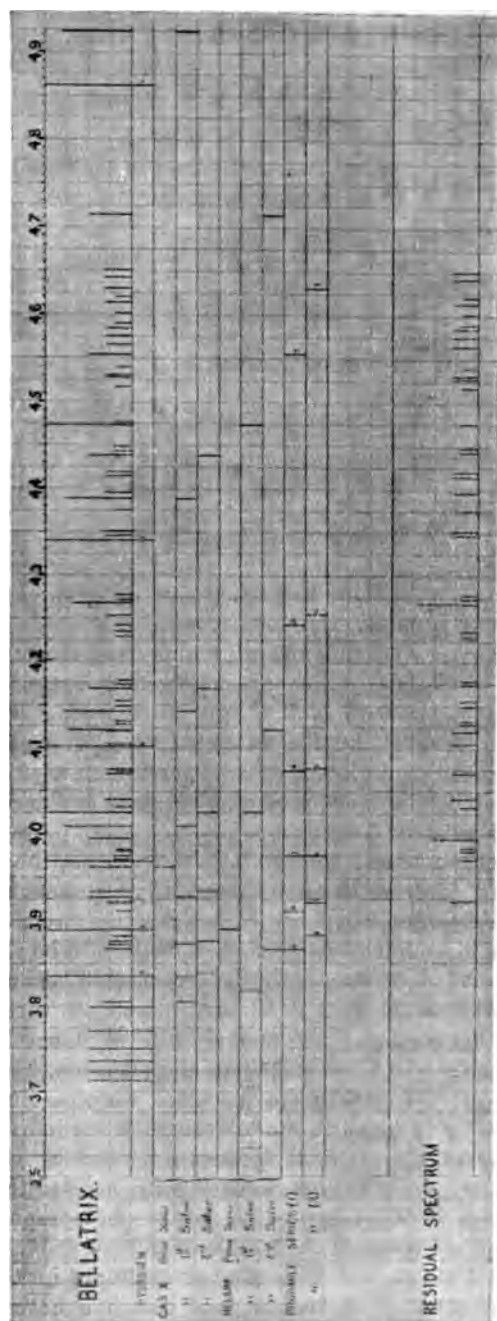
The accompanying map shows the principal lines in the spectrum
of Bellatrix, and indicates those which are due to hydrogen, helium,
and asterium. The two probable new series which have been found
by Dr. Lockyer are also shown.

The New Gas in ζ Puppis.

Professor E. C. Pickering has recently announced[*] that the
spectrum of ζ Puppis contains a new series of lines, which he at first
supposed to be due to some new element. In a second communica-
tion to the same journal,[†] he pointed out that he had reason to
suppose that this new series was in some way connected with
hydrogen, since he found that the observed lines occupied the same
positions as those computed from the same formula and constants,
from which the ordinary series of hydrogen was calculated, but using
odd values of n instead of even values.

In the following table I have brought together all the lines
published by Professor Pickering as belonging to the spectrum of
this star, ranging them in different columns for greater clearness:—

Lines in the Spectrum of ζ Puppis.

Hydrogen.		Other lines with origins.	
Old Series (dark).	New Series (dark).	Dark.	Bright.
3798·1	3783·4	3933 Ca	4698 unknown
3835·5	3815·9	4472 He	5652 ,,
3889·1	3858·6	4505 unknown	
3970·2	3924·8	4620 ,,	
4101·8	4026·8	4633 ,,	
4340·7	4200·4	4688 ,,	
4861·5	4544·0		

In his first communication, Professor Pickering mentions lines at
4698, 4652, 4620, and 4505, but he does not refer to the first three in
his second paper. The line 4505 was at first taken to be one of the
components of the new series, but this seems to have been subse-

* 'Astrophysical Journal,' vol. 4, p. 369.
† 'Astrophysical Journal,' vol. 5, p. 95.

quently superseded by the employment of the line about 4544, which agrees better both as regards intensity and the calculated position 4543·6.

The question then arises, what relation does the spectrum of this star bear to those of other stars of high temperature?

A comparison of the lines recorded with those in the spectrum of Bellatrix shows that, with the exception of the new series and the lines at 4698, 4688, and 4505, many lines are common, as is indicated in the following table :—

Comparison of Spectrum of ζ Puppis with that of Bellatrix.

Lines in ζ Puppis (Pickering).	Probable coincident lines in Bellatrix.	Origins.
3783·4	—	H new series ($n = 21$).
3798·1	3798·1	Hθ.
3815·9	—	H new series ($n = 19$).
3835·5	3835·5	Hη.
3858·6	—	H new series ($n = 17$).
3889·1	3889·1	Hζ.
3924·8	—	H new series ($n = 15$).
3933·0	3933·0	K.
3970·2	3971·1	Hε.
4026·8	—	H new series ($n = 13$).
4101·8	4101·0	Hδ.
4200·4	—	H new series ($n = 11$).
4340·7	4340·7	Hγ.
4472·0	4471·0	Helium.
4505·0	—	Unknown.
4540·0	4541·0	Unknown.
4544·0	—	H new series ($n = 9$).
4620·0	4620·0	Unknown.
4633·0	4629·0	Unknown.
4652·0	4650·0	Unknown.
4688·0	—	Unknown.
4698·0	—	Unknown.
4861·5	4861·0	Hβ.

From the above it will be gathered that the only really marked difference between ζ Puppis and Bellatrix is the presence of this new series of lines in the spectrum of the former. As I have only at my command the published accounts of Professor Pickering and not an original photograph of this star, a more detailed comparison of the spectra cannot be made, but there seems to be evidence which points to a higher temperature for the star than that of Bellatrix.

Professors Pickering and Kayser both concede that this new form

of hydrogen is due most probably to a high temperature, and Professor Kayser expressly states that "this series has never been observed before, and can perhaps be explained by insufficient temperature in our Geissler tubes and most of the stars."[*] I pointed out in my former paper that this new series and the one previously known are probably of the subordinate type, and that the principal series is still unrecognised,[†] although some of the "unknown" lines in stars may possibly belong to it.

On the supposition that the new series of probable hydrogen lines in ζ Puppis represents the effect of a transcendental temperature, an attempt has been made to produce this spectrum in the laboratory. In the high-tension spark in hydrogen at atmospheric pressure the ordinary series of hydrogen lines is very broad, and none of the new series have so far been detected. The use of the spark with large jars in vacuum tubes results in the partial fusion of the glass and the appearance of lines which have been traced to silicium, while the new series has not yet been observed.

Final Result as to Temperature.

In the preliminary attempt to determine which are the hottest stars, the following facts and deductions have been considered :—

1. With increasing temperature hydrogen is first visible, then helium and asterium appear nearly together, and finally unknown lines at λλ 4088·7 and 4650·9 make their appearance.

2. The chief helium lines in the region covered by the photographs become much thicker after the α Cygni stage has been passed, and are practically of equal thicknesses in the stars Bellatrix, Spica, ζ̃ Orionis, ζ Orionis, ε Orionis, and β Persei, after which a sudden diminution in intensity takes place. These lines give us *no* criterion for the *hottest* star of the series.

3. With regard to the chief lines of asterium, namely, 4008·7 and 4388·1, in the region under investigation, these both rise to a very decided maximum in Bellatrix, diminishing afterwards in intensity less rapidly than they increased. The great development of asterium after the lines of helium have reached a considerable thickness suggests a higher temperature for Bellatrix than the neighbouring stars in the series.

4. As asterium begins to decrease in intensity, the two unknown lines before referred to at λλ 4088·7 and 4650·9 commence to brighten, reaching a maximum at ε Orionis, in which the hydrogen lines are still at a maximum, but asterium has considerably decreased. Only a trace, if any, of these lines can be found in Bellatrix. If

[*] 'Astrophysical Journal,' vol. 5, p. 96.
[†] 'Roy. Soc. Proc.,' vol. 61, p. 195.

these lines when at maximum are indications of a higher tempera-
ture than those of asterium, then, since the hydrogen and helium
lines are also at a maximum, ϵ Orionis on this assumption would be
the hottest star of the series.

With our present data it is, however, difficult to state with certainty
whether the principal series of helium or of asterium makes its
appearance first.   There seem, however, to be indications which
suggest that asterium is a somewhat later development.

In summing up I may say that ϵ Orionis may be considered the
hottest star from the behaviour of the lines at 4481·3, 4088·7, 4650·9,
while 4008·7, 4267·6, and 4388·1 favour the star Bellatrix in this
respect.

The helium lines (4026·3 and 4471·6) have practically the same
intensity in both stars, or at any rate there is not sufficient difference
to serve as a criterion.

General Conclusions.

1. The order of temperature of stars at and near the apex of the
temperature curve can only be determined by reference to unknown
lines, since the enhanced lines of iron are absent, and those of mag-
nesium and calcium are exceedingly feeble, while the lines of the
known gases show no very marked variations.

2. The varying appearances of the lines of the cleveite gases indi-
cate, as laboratory work has done, that helium and gas X are distinct
substances, but there is not yet sufficient evidence for regarding the
constituent series as belonging to separate substances. It is therefore
considered that gas X should be definitely named, and the name
" asterium " is suggested.

3. There are two methods open to us for discriminating between
gaseous and metallic lines of still unknown origin in the spectra of
the hottest stars.   (a) Gaseous lines like those of helium and hydro-
gen will be common to nebulæ and the hottest stars.   (b) Metallic
lines like those of iron, magnesium, and calcium will thin out at
increased stellar temperature, while gaseous lines will become inten-
sified.

4. Several unknown lines in the spectra of the hottest stars are
thus shown to be most probably of gaseous origin.

5. Attempts to trace terrestrial sources of these stellar gases have
resulted in the detection of lines which probably coincide with lines
in the spectra of the hottest stars.

6. On the supposition that these stellar gases are more or less
allied to helium and asterium, since they have their maximum inten-
sity in the same stars, attempts have been made to trace " series "
of lines in the spectra.   In the case of Bellatrix two probable series
have already been recognised.

7. The new series of probable hydrogen lines in ζ Puppis most likely represents the effect of a transcendental temperature. This and the well-known series are in all probability of the subordinate type, the lines of the principal series not yet having been identified in stars.

8. There is evidence which points to a higher temperature for ζ Puppis than for Bellatrix.

9. The behaviour of certain lines suggests that Bellatrix may be taken as a type of the hottest stars, while the behaviour of others seems to indicate that ϵ Orionis should be regarded as a star of the very highest temperature, exception being made of ζ Puppis. There are not yet sufficient data to enable a final statement to be made.

"A Maya Calendar Inscription, interpreted by Goodman's Tables." By ALFRED P. MAUDSLAY. Communicated by F. DUCANE GODMAN, F.R.S. Received April 2,—Read June 17, 1897.

[Introductory Note.

Our knowledge of the Maya Calendar is chiefly derived from the writings (A.D. 1566) of Diego de Landa, Bishop of Yucatan, who not only gave some account of the divisions of time in use among the Mayas, but also copied, somewhat roughly, in his manuscript the signs employed to represent the eighteen named months, and the twenty named days into which each month was divided.

Landa's statements are, however, by no means clear, and there has been much discussion both as to their correctness in themselves and as to the interpretation which has been given to them; moreover, it has been found difficult in some instances to identify the day and month signs given by him with those used in the Dresden Codex and the few Maya manuscripts which have been preserved, and still more difficult to identify them with the signs used in the carved inscriptions.

In the accompanying paper an examination is made of a recently discovered inscription, by the aid of calendar tables prepared by Mr. J. T. Goodman, and published with an explanatory essay in the 'Biologia Centrali-Americana.' These tables consist of a chronological and an annual calendar. The chronological calendar is based on the Ahau, a period of 360 days, and is divided thus :—

$$20 \text{ days} \quad \dots\dots\dots \quad 1 \text{ chuen}$$
$$18 \text{ chuens} \quad \dots\dots\dots \quad 1 \text{ ahau* (360 days)}$$

* It is unfortunate that the *ahau*, or period of 360 days, bears the same name as one of the twenty days of the Maya month, and that the *chuen*, or twenty-day period, bears the name of another day of the month.

20 ahaus .... 1 katun
20 katuns.......... 1 cycle
13 cycles .... 1 great cycle
73 great cycles .... 1 grand era

The annual calendar is divided into eighteen named months, each consisting of twenty named days, and one short month (named Uayeb) of five days.

The twenty named days of the month are numbered continuously from 1 to 13, so that if the first-named day of the month has the number 1 attached to it, the last-named day of the month will be numbered 7 (13+7 = 20), and the first day of the next month will be numbered 8, and so on.

There are fifty-two annual calendars in a calendar round, and at the end of the 52nd year the series is repeated.

All the dates and reckonings found on the monuments which can be made out by the aid of these tables are expressed in Ahaus, Katuns, &c., and not in years; but Mr. Goodman maintains that the true year was known to the Mayas, and that it is by the concurrent use of the chronological and annual tables that the dates carved on the monuments can be properly located in the Maya Calendar.

All the dates which have as yet come under notice fall within the three Great Cycles, numbered by Mr. Goodman the 53rd, 54th, and 55th.

The following extract from an article in 'Nature' (July 8, 1897) gives a good example of the manner in which a date is expressed :—

"I called attention, some years ago, to the fact that the greater number of the carved inscriptions commenced with easily recognised series of glyphs with numerals or faces attached to them, which I called the Initial Series. Mr. Goodman now shows that the Initial Series expresses a date thus :—

1.   2.   3.   4.

5.   6.   7.   8.
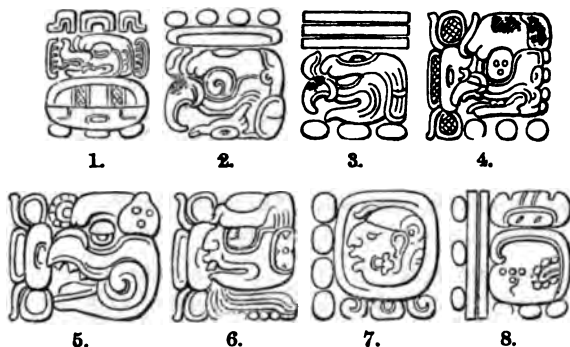
(1) The Great Cycle sign. (2) The Cycle. (3) The Katun. (4) The Ahau.
(5) The Chuen.   (6) The Day.   (7) The named day.   (8) The named month.

As has been long known, each bar counts as five, and each dot as a unit. (The roundish marks *under* the glyphs are not part of the numerical series.)

"The signs in front of the Ahau, Chuen, and Day signs denote a 'full count' of those periods. The date thus reads :—

(1.)	54th................	great cycle.
(2.)	9th................	cycle.
(3.)	15th................	katun.
(4.)	'Full count'	ahaus.
(5.)	'Full count'	chuens.
(6.)	'Full count'	days.
(7.)	4 Ahau (day).	
(8.)	13 Yax (month).	

"A reference to Mr. Goodman's chronological calendar shows that the 15th Katun of the 9th Cycle of the 54th Great Cycle commences with the day 4 Ahau, the 13th day of the month Yax, the date which is here given in the inscription. The combination 4 Ahau 13 Yax can only occur once in a period of fifty-two years.

"One of Mr. Goodman's discoveries is the system on which the Mayas numbered the different series of time divisions. For instance, the twenty Ahaus are not numbered 1, 2, 3, &c., up to 20, but they were numbered 20, 1 2, 3, &c., to 19.

"If we should nowadays wish to use a similar notation, we should probably number the series 0, 1, 2, &c., 19 ; but it seems as though the Mayas, having no sign for 0, wrote the sign for 20 or a 'full count' of Ahaus in the first place.

"The eighteen Chuens are in like manner numbered 18, 1, 2. 3, &c., to 17, the same sign being used for a 'full count' of Chuens as is used for a 'full count' of Ahaus.

"As a 'full count' of days (twenty) is a Chuen, a 'full count' of Chuens (eighteen) is an Ahau, and a 'full count' of Ahaus (twenty) is a Katun. The foregoing inscription may be read thus :—

"The 15th Katun of the 9th Cycle with no odd Ahaus, Chuens, or days added, begins with 4 Ahau 13 Yax.

"Had the date been one including a specified number of Ahaus, Chuens, or Days, we should have had to make use of the annual calendar.

"The faces so frequently met with in the inscriptions in connexion with Cycle, Katun, and other signs for time periods are shown by Mr. Goodman to be in reality numerals, and the whole series of numeric faces from 1 to 20 has been determined in some cases with certainty, and in others with a fair degree of probability."—August 5, 1897.]

In the month of February, when the last pages of Mr. Goodman's
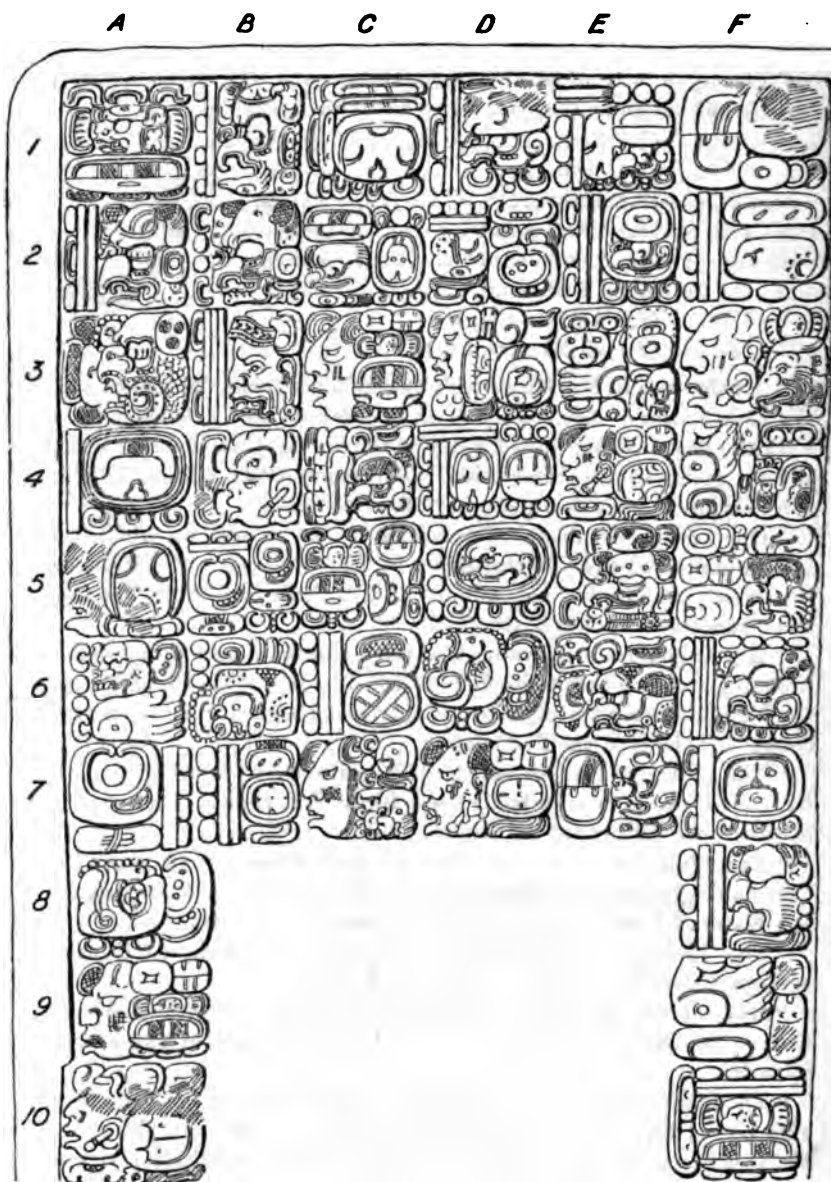
Maya Inscription from Piedras Negras.

(The glyphs are read downwards in double columns from left to right.)

essay (published in the Archæological Section of the 'Biologia Centrali-Americana') were issuing from the press, I received from Mr. Teobert Maler a number of photographs of sculptures and inscriptions which he had recently discovered in Yucatan and the country to the south of it as far as the banks of the River Usumacinta.

One of these inscriptions from Piedras Negras on the Usumacinta is in a good state of preservation, and a drawing made by Miss Annie Hunter from the photographic print is here reproduced (p. 70).

As Mr. Goodman has never seen this inscription, an examination of it with the help of his notes and calendar tables will be a fair test of their value.

The following signs are figured in Mr. Goodman's essay, and will be found to agree fairly well with those in the inscription.

Cycle

= 20 Katuns.

Katun
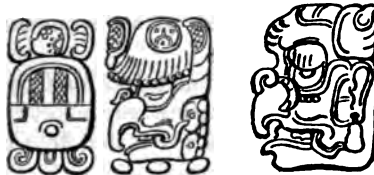
= 20 Ahaus.

Ahau

= 18 Chuens.

Chuen
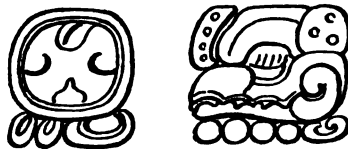
= 20 Days.

Signs for the named days—

Cimi.   Cib.   Ymix.   Ahau.

Signs for the named months—

Yaxkin.   Kankin.   Uo.   Yax.   Muan.

The glyph A 1 is the initial glyph indicating the Great Cycle.   It has more the appearance of the sign for the 53rd than for that of the 54th Great Cycle, but the signs for the different Great Cycles are still in need of elucidation, and the subsequent reckoning shows clearly that the dates fall within the table given by Mr. Goodman as that of the 54th Great Cycle.

The next glyph B 1 is the Cycle sign with the numeral 9 in front of it (one bar = 5 and four dots = 4).

A 2 is the Katun sign with the numeral 12 in front of it (two bars = 10, and two dots = 2; the hollow curve between the two round dots is merely used to fill up the space, and does not count).

B 2 is the Ahau sign with the numeral 2.

Turning to the tables of Mr. Goodman's chronological calendar, of which an extract showing the 10th to the 14th Katuns of the 9th Cycle is here given, we find that the first day of the—

> 2nd Ahau,
> 12th Katun,
> 9th Cycle,
> 54th Great Cycle,

falls on the day 2 Ahau, the 18th day of the month Xul (which is underlined in the table).

This is as far as the chronological calendar can guide us.   We have next to find the position of this date in the annual calendar. The date can only occur once in the fifty-two years which constitute a calendar round, and an examination of the tables shows that it falls in the first year of the calendar round (where it is marked with a square).

Fifty-fourth Great Cycle. Ninth Cycle.

Nos. of the Katuns.. 10

No. of the Ahau	14 — No. of the day Ahau	14 — Day of the month	14 — Name of month	13 — No. of the day Ahau	13 — Day of the month	13 — Name of month	12 — No. of the day Ahau	12 — Day of the month	12 — Name of month	11 — No. of the day Ahau	11 — Day of the month	11 — Name of month	10 — No. of the day Ahau	10 — Day of the month	10 — Name of month	No. of the Ahau
20	6	13	Muan	8	8	Uo	10	8	Yaxkin	12	8	Ceh	1	8	Kayab	20
1	2	8	„	4	3	„	6	3	Xul	8	3	Zac	10	8	Pax	1
2	11	3	„	13	18	Pop	2	18	„	4	18	„	6	18	„	2
3	7	18	Kankin	9	13	„	11	13	„	13	13	„	2	13	„	3
4	3	13	„	5	8	„	7	8	„	9	8	Yax	11	8	Muan	4
5	12	8	„	1	3	„	3	3	Tzec	5	3	„	7	18	„	5
6	8	3	Mac	10	3	Uayeb	12	18	„	1	18	„	3	13	„	6
7	4	18	„	6	18	Cumhu	8	13	„	10	13	Chen	12	8	Kankin	7
8	13	13	„	2	13	„	4	8	„	6	8	„	8	3	„	8
9	9	8	Ceh	11	8	„	13	3	Zotz	2	3	„	4	18	„	9
10	5	3	„	7	3	Kayab	9	18	„	11	18	Mol	13	13	Mac	10
11	1	18	„	3	18	„	5	13	„	7	13	„	9	8	„	11
12	10	13	„	12	13	„	1	8	„	3	8	„	5	3	„	12
13	6	8	Zac	8	8	Pax	10	3	Zip	12	3	„	1	18	Ceh	13
14	2	3	„	4	3	„	6	18	„	8	18	Yaxkin	10	13	„	14
15	11	18	„	13	18	„	2	13	„	4	13	„	6	8	„	15
16	7	13	„	9	13	Muan	11	8	Uo	13	8	„	2	3	„	16
17	3	8	„	5	8	„	7	3	„	9	3	„	11	18	„	17
18	12	3	„	1	3	„	3	18	„	5	18	„	7	13	„	18
19	8	18	Yax	10	18	Muan	12	13	„	1	13	„	3			19

Archaic Annual Calendar. 1st Year.

	Names of the days	Pop	Uo	Zip	Zotz	Tzec	Xul	Yaxkin	Mol	Chen	Xax	Zac	Ceb	Mac	Kankin	Muan	Pax	Kayab	Cumhu	Uayeb
20	Ik	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10
1	Akbal	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11
2	Kan	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12
3	Chicchan	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13
4	Cimi	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1
5	Manik	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	
6	Lamat	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	
7	Muluc	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	
8	Oc	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	
9	Chuen	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	
10	Eb	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	
11	Ben	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	
12	Ix	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	
13	Men	1	8	2	9	3	10	(5)	11	5	12	6	13	7	1	8	2	9	3	
14	Cib	2	9	3	10	4	11	6	12	6	13	7	1	8	2	9	3	10	4	
15	Caban	3	10	4	11	5	12	7	13	7	1	8	2	9	3	10	4	11	5	
16	Ezenab	4	11	5	12	6	13	8	1	8	2	9	3	10	4	11	5	12	6	
17	Cauac	5	12	6	13	7	1	9	2	9	3	10	4	11	5	12	6	13	7	
18	Ahau	6	13	7	1	8	[2]	10	3	10	4	11	5	12	6	13	7	1	8	
19	Ymix	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	

The next glyph in the inscription A 3 is the Chuen sign with the sign which signifies a "full count" of Chuens, in front of it. As a full count of Chuens is 18 and equals 1 Ahau, and as the number of Ahaus has already been recorded, the glyph A 3 means that no odd Chuens are to be added to the date already expressed.

The glyph B 3 is the sign for a day (of twenty-four hours) preceded by the numeral 16.

Turning to the first year of the annual calendar, we now add these 16 days to 2 *Ahau* 18 *Xul*, the date already arrived at, and it will be found to bring us to 5 *Cib* 14 *Yaxkin* (marked with a circle).

That this reckoning is correct is shown by the inscription itself where the result is expressed; A 4 being 5 *Cib*, and B 7 14 *Yaxkin*. The six glyphs in the inscription intermediate between the sign of the day Cib, and the sign of the month Yaxkin, have not yet been thoroughly deciphered, but there is reason to suppose that they contain a parallel reckoning differently expressed.

The next three glyphs are undeciphered; then comes another reckoning :—

C 1 is the Chuen sign with the numeral 10 (two bars = 10) above it, and a "full count" sign at the side. Whether the 10 applies to the Chuens or days can only be determined by experiment, and such experiment in this case shows that the reckoning intended to be expressed is 10 Chuens and a "full count" of days, that is for practical purposes 10 Chuens only, for as in the last reckoning when the full count of Chuens was expressed in the Ahaus, so here the full count of days is expressed in the Chuens.

The next glyph D 1 is an Ahau sign, preceded by the numeral 12. This gives us—

$$12 \text{ Ahaus } (12 \times 360) = 4320 \text{ days.}$$
$$10 \text{ Chuens } (10 \times 20) = 200 \text{ „}$$

$$\overline{4520 \text{ days.}}$$
$$4380 \text{ „ } = 12 \text{ years.}$$

$$\overline{140 \text{ days.}}$$

Adding 4520 days, or 12 years and 140 days, to the date 5 *Cib* 14 *Kankin*, it brings us to the date 1 *Cib* 14 *Kankin* in the thirteenth year of the annual calendar.

Turning to the inscription we find at C 2 (passing over the first half of the glyph), 1 *Cib* followed by (the first half of D 2) 14 *Kankin*, the date at which we have already arrived by computation.

13th Year.

Pos	Names of the days	Pop	Uo	Zip	Zotz	Tzec	Xul	Yaxkin	Mol	Chen	Yax	Zac	Ceh	Mac	Kankin	Muan	Pax	Kayab	Cumhu	Uayeb
20	Ik	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9
1	Akbal	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10
2	Kan	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11
3	Chicchan	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12
4	Cimi	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13
5	Manik	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	:
6	Lamat	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	:
7	Muluc	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	:
8	Oc	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	:
9	Chuen	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	:
10	Eb	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	:
11	Ben	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	:
12	Ix	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	:
13	Men	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	:
14	Cib	1	8	2	9	3	10	4	11	5	12	6	13	7	(1)	8	2	9	3	:
15	Caban	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	:
16	Ezenab	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	:
17	Cauac	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	:
18	Ahau	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	:
19	Ymix	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	:

Names of the months

35th Year.

	Names of the days	Pop	Uo	Zip	Zotz	Tzec	Xul	Yaxkin	Mol	Chen	Yax	Zac	Ceh	Mao	Kankin	Muan	Pax	Kayab	Cumhu	Uayeb
20	Eb	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5
1	Ben	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6
2	Ix	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7
3	Men	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8
4	Cib	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9
5	Caban	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	:
6	Ezanab	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	:
7	Cauac	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	:
8	Ahau	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	:
9	Ymix	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	:
10	Ik	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	:
11	Akbal	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	:
12	Kan	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	:
13	Chicchan	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	:
14	Cimi	10	(4)	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	:
15	Manik	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	:
16	Lamat	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	:
17	Muluc	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	:
18	Oc	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	:
19	Chuen	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	:

38th Year.

	Names of the days.	Pop.	Uo.	Zip.	Zotz.	Tzec.	Xul.	Yaxkin.	Mol.	Chen.	Yax.	Zac.	Ceh.	Mac.	Kankin.	Muan.	Pax.	Kayab.	Cumhu.	Uayeb.	
20	Manik	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	20
1	Lamat	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	1
2	Muluc	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	2
3	Oc	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	3
4	Chuen	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	4
5	Eb	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	:	5
6	Ben	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	:	6
7	Ix	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	:	7
8	Men	7	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	:	8
9	Cib	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	:	9
10	Caban	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	:	10
11	Ezenab	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	:	11
12	Cauac	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	:	12
13	Ahau	12	6	13	7	1	8	2	9	3	10	4	11	5	12	(6)	13	7	1	:	13
14	Ymix	13	7	1	8	2	9	3	10	4	(11)	5	12	6	13	7	1	8	2	:	14
15	Ik	1	8	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	:	15
16	Akbal	2	9	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	:	16
17	Kan	3	10	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	:	17
18	Chicchan	4	11	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	:	18
19	Cimi	5	12	6	13	7	1	8	2	9	3	10	4	11	5	12	6	13	7	:	19

Names of the months ……

Passing over the next three glyphs we arrive at another reckoning, D 4 gives 10 days, 11 Chuens, 1 Ahau, and the first half of C 5 gives 1 Katun.

1 Katun	7200 days.
1 Ahau	360 ,,
11 Chuens (11×20)	220 ,,
10 Days	10 ,,

7790 days.
7665 ,, = 21 years.

125 days.

Adding 7790 days or 21 years and 125 days to the previous date, 1 *Cib* 14 *Kankin*, it will bring us to 4 *Cimi* 14 *Uo* in the thirty-fifth year of the annual calendar, and we find this date expressed in the inscription in the glyphs D 5 and C 6.

Passing over the next three glyphs we arrive at another reckoning (E 1), 3 Ahaus, 8 Chuens, 15 days :—

3 Ahaus	1080 days.
8 Chuens........	160 ,,
15 Days..........	15 ,,

1255 days.
1095 ,, = 3 years.

160 days.

Adding 3 years and 160 days to the last date, 4 *Cimi* 14 *Uo*, brings us to 11 *Ymix* 14 *Yax* in the thirty-eighth year of the annual calendar; this is the date we find expressed in the glyphs E 2 and F 2 of the inscription.

It is true that in the sign in the glyph E 2 is not the sign usually employed for the day Ymix, but that it is a day sign we know from the fact that it is included in a cartouche, and I am inclined to think that the more usual Ymix sign (something like an open hand with the fingers extended) was inclosed in the oval on the top of the grotesque head, but it is too much worn for identification.

Passing over seven glyphs, the next reckoning occurs at F 6, which gives :—

4 Chuens	80 days.
19 Days	19 ,,

99 days.

Adding 99 days to the last date, 11 *Ymix* 14 *Yax*, brings us to 6 *Ahau* 13 *Muan* in the same year, and we find this date expressed in F 7 and F 8.

The last glyph in the inscription is a Katun sign with the numeral 14 above it, and a sign for " beginning " in front of it, and indicates that the last date is the beginning of a 14th Katun.   If we turn to the table for the 9th Cycle of the 54th Great Cycle, from which we started, it will be seen that the 14th Katun of that cycle does commence with the date 6 *Ahau* 13 *Muan*.

It is simply impossible that the identity of the dates expressed in the inscription with those to which the computations have guided us can throughout be fortuitous.   Very nearly half of the forty-eight glyphs in the inscription have been accounted for, and I have no doubt that when the inscription passes under Mr Goodman's scrutiny he will be able to give us much information about the remaining glyphs which I have passed over as undeciphered.

It can, I think, therefore, be fairly claimed for Mr. Goodman that his researches have raised the veil of mystery which has for so long hung over the carved hieroglyphic writing of the Mayas.

" Influence of Acids and Alkalis upon the Electrotonic Currents of Medullated Nerve."   By AUGUSTUS D. WALLER, M.D., F.R.S.   Received June 10,—Read June 17, 1897.

A. *The Effect of Acids and of Alkalis.*

Considering that electrotonic currents are characteristic of living medullated nerve, that such currents are due to electrolytic polarisation, and that such electrolysis must primarily consist in a liberation of electronegative principles (oxygen, acid, &c.) at the anode, and of electropositive principles (hydrogen, base, &c.) at the kathode, the first and most obvious test to be made is to examine comparatively the action of acids and bases upon anelectrotonic and katelectrotonic currents.

On the supposition that a medullated nerve-fibre is composed of two different electrolytes, white fatty sheath and grey proteid axis, and that electrolytic polarisation is aroused at the interface of separation between these two electrolytes, we may expect to find, as the characteristic acidic effect, diminution of A and increase of K, and as the characteristic basic effect, increase of A and diminution of K.

This expectation is in the main substantiated by experiment, although owing to the somewhat narrow range of concentration within which moderate effects are produced, it is not common to obtain effects in both of the two opposite directions in a single experiment.   The reagent may be too weak, in which case neither A nor K are altered, or it may be too strong, in which case both A
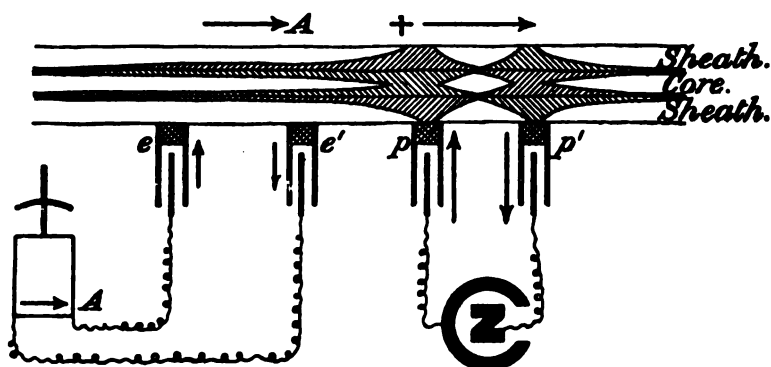
FIG. 1.
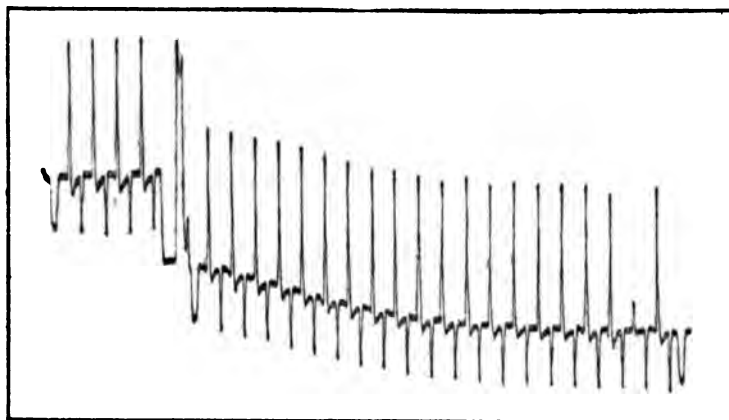
FIG. 2.—Potassium hydrate, N/50 (2358).
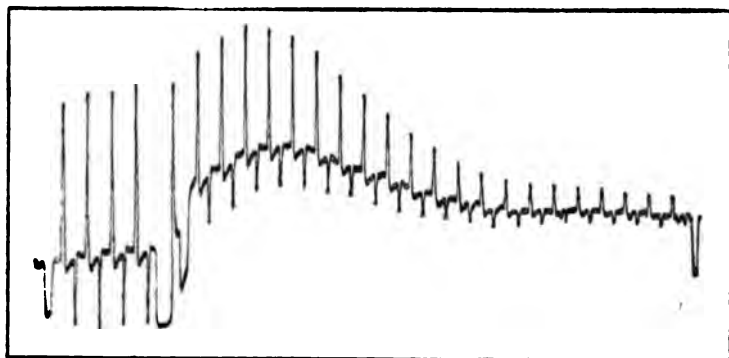
FIG. 3.—Effect of sulphuric acid, N/5 upon A and K (2359).

and K are rapidly and equally abolished. Plates 2358 and 2359
illustrate this point, the former exhibiting the defective action of

a base below optimum strength, the latter exhibiting the excessive action of an acid above optimum strength.

Partly for this reason, and partly in order to eliminate the resistance factor, results are formulated in terms of the relative magnitude of the quotient A/K as well as in terms of the absolute magnitudes of A and K.   This point is illustrated by plate 2410.
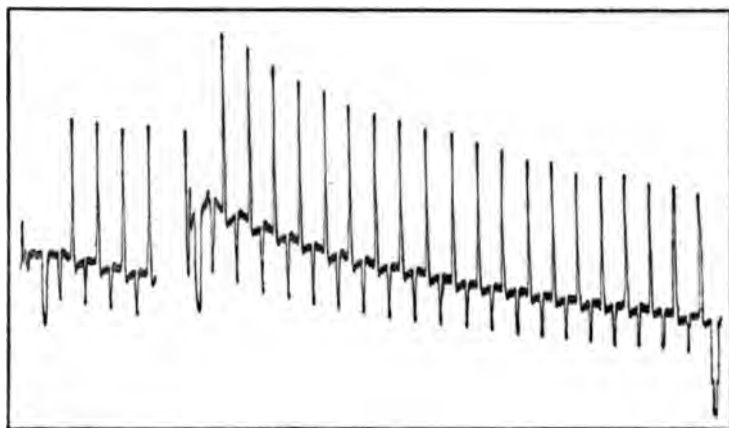
FIG. 4.—Oxalic acid N/20 (2410).

Method.—The disposition of the object of experiment is in accordance with the diagram, and the galvanometer (dead-beat) is arranged
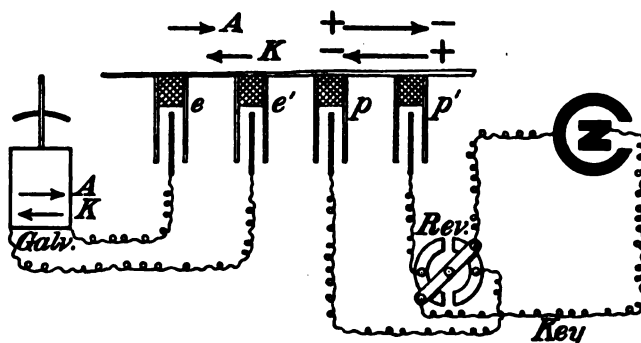
FIG. 5.—Diagram of apparatus. The excised nerve rests upon two pairs of unpolarisable electrodes, *pp'* leading in the polarising current, *ee'* leading out the extrapolar or electrotonic current.

to give a continuous record (lasting usually from 30 to 60 minutes) as described in a previous communication (Croonian Lecture, 1896, ' Phil. Trans.,' B, 1897).

In the earlier observations series of anelectrotonic and of katelectrotonic currents were separately recorded. In the later observations the A and K currents were taken at alternate minutes by means of a rotating reverser in the polarising circuit. In the finished records A currents read upwards, and K currents read downwards.

Results.—The characteristic results of acid and base upon the anodic and kathodic currents respectively are summarised in the following four observations (Plates 2360, 2412, 2429, 2432).
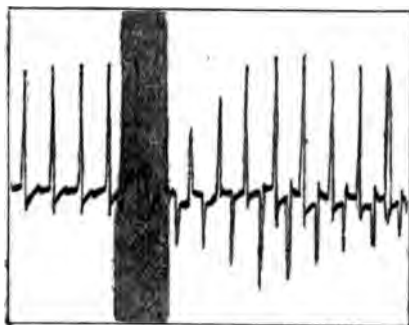
Fɪɢ. 6.—Action of CO_2 on A and K (2429).
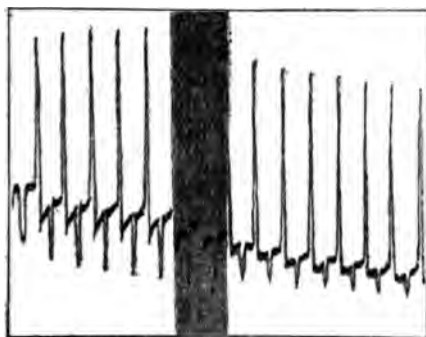
Fɪɢ. 7.—Action of ammonia vapour on A and K (2432).

They illustrate the rule that:

Acidification diminishes the quotient A/K.
Basification increases tho quotient A/K.

A diminution of the quotient A/K may be by diminution of A or by increase of K. In plate 2412 it is mainly by diminution of A. In this case the augmentation of K is comparatively small. The record in fact approaches towards the type of plate 2359. In other
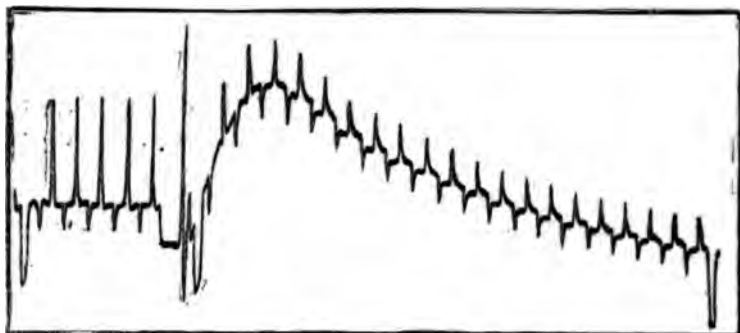
FIG. 8.—Effect of propionic acid, N/10, upon A and K (2412).
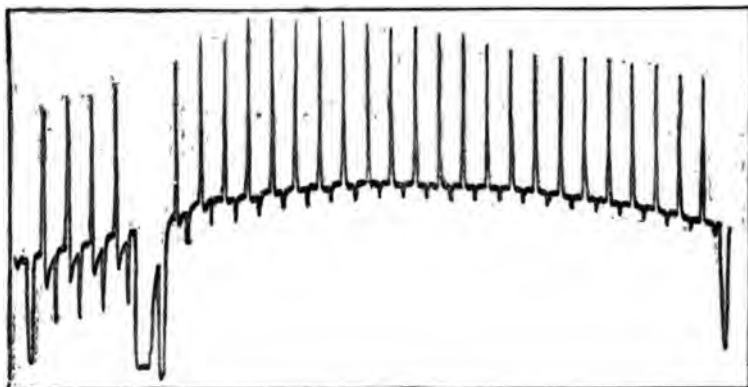
FIG. 9.—Effect of a weak alkaline bath (KOH N/20 or 0·285 per 100) upon A and K (2360).

experiments (*e.g.*, in plates 2429 and 2424) the diminution of A/K is principally due to increase of K.

An increase of the quotient A/K may be by increase of A or by diminution of K. In plate 2360, and in most of my other experiments, it is mainly by diminution of K.

The two plates illustrate the points that acid affects A more than it does K, and that base affects K more than it does A.

As mentioned above, acid above optimum strength causes a diminution of K; we may, therefore, say that weak acid causes augmented K, and stronger acid diminished K.

On testing carefully with very weak acids we shall find that at a strength below the optimum (giving diminished A and increased K, *e.g.*, Plate 2429) the "very weak" acid causes augmented A. We may, therefore, say that very weak acid causes augmented A and rather stronger acid diminished A. And in general summary of the action of acid from minimal to maximal effective we may state that:—

(1) The weakest acid gives increased A.

(2) Slightly stronger acid gives diminished A and increased K.

(3) Still stronger acid gives diminished K.

These statements are the outcome of a considerable number of observations, and one may hardly hope to verify the progressive action of acid from minimal to maximal in a single observation with a single acid. Nor is it easy to give numbers in lieu of the indefinite qualifications "weak" and "strong." This much may, however, be said to give an idea of the order of magnitudes dealt with. The second degree of change may be expected in consequence of bathing the nerve for one minute in an acid solution of a strength between N/20 and N/10. The free passage of "much" CO_2 into the nerve-chamber usually affects the second degree of change in its most typical form. A small amount of CO_2, *e.g.*, a few puffs of expired air, will more probably affect the first degree of change. A bath of one minute's duration in a N/5 solution of mineral acid will almost certainly affect the third degree of change. A diminution of K by CO_2 is rare (*e.g.*, 2363).

B. *The Effect of Carbonic Acid and of Tetanisation.*

I have given particular attention to the action of carbon dioxide and of tetanisation upon the A and K currents, in prosecution of observations already reported concerning the action-currents of nerve* and the influence of temperature upon the A and K currents.

The usual and typical effects of carbonic acid are of the characteristic acidic type, consisting in a diminution of A and an augmentation of K.
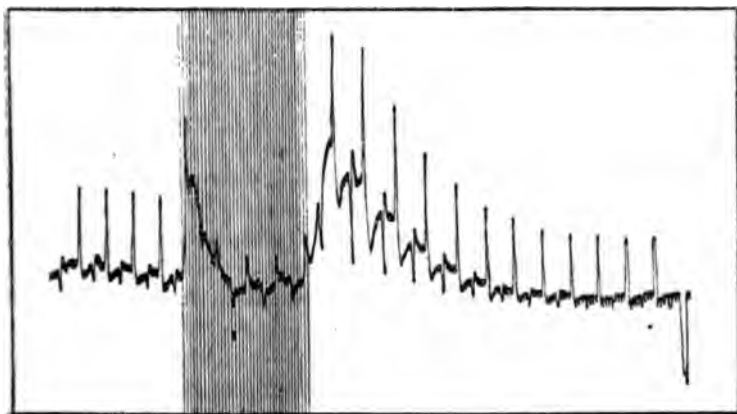
Fig. 10.—Action of CO_2 upon A and K (2422).

* 'Phil. Trans.,' B, 1897, p. 1.

Less commonly, and by a slighter degree of action of CO_2, the A current may be increased, while as the most pronounced degree of action of CO_2 the K current may be diminished.

In order of gravity the effects are :

    1. Augmentation of A.

    2. { Diminution of A.

        { Augmentation of K.

    3. Diminution of K.

The second being the usual and typical result, the first and third being less freqnently observed.

Prolonged tetanisatiou (five minutes) modifies the A and K currents in a similar direction, causing a diminution (but sometimes an augmentation) of the A current and an augmentation (nearly alwjys) of the K current (2424).
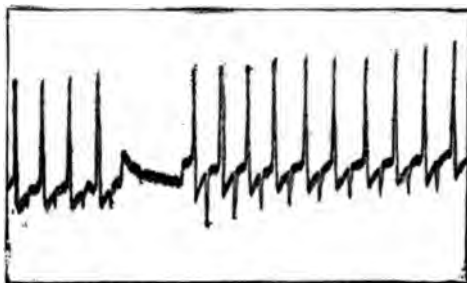
FIG. 11.—Effect of tetanisation on A and K (2424).

Thus it will be seen that the two groups of results, although not absolutely coincident, are in reasonable agreement, the two points of difference being that an augmentation of the A current has been more frequent by tetanisation than by CO_2; while a diminution of the K current, rarely observed in consequence of the full action of CO_2, has been still more rare (once only, and that not very markedly, 2287) in consequence of tetanisation.

Of these several effects the most characteristic has been the augmeutation of K (2424, fig. 11) with a consequent diminution of the quotient A/K. And although—in correspondence with the not infrequent augmentation of A, there has been not infrequently an augmentation of A/K—this latter augmentation has generally been slight or even doubtful as compared with its opposite. I have been led to admit diminution of A/K as typical (2424, fig. 11, 2425 2427), and a distinct augmentation of A/K as exceptional (2387, 2388, 2393) or doubtful. (A similar augmentation of A/K by predominant augmeuhtation of A has not hitherto come under my observation in consequence of the action of CO_2.)
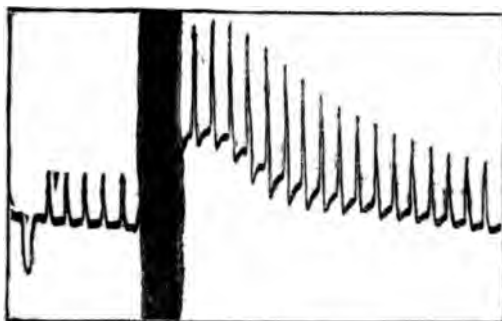
FIG. 12.—Effect of CO_2 on A (primary augmentation) (2199).
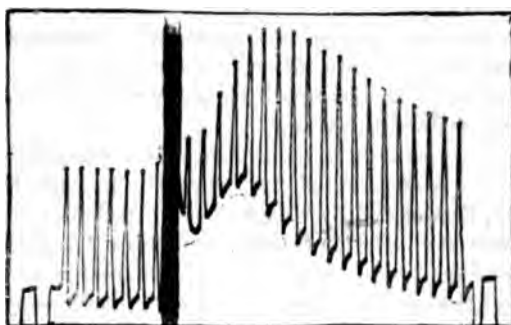
FIG. 13.—Effect of CO_2 on A (primary diminution, secondary augmentation) (2200).
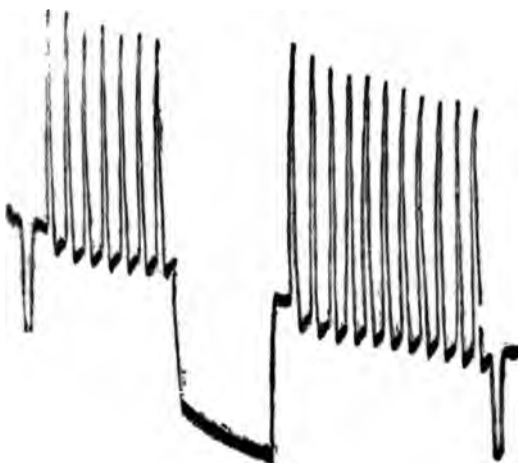
FIG. 14.—Effect of tetanisation on A (augmentation) (2295).
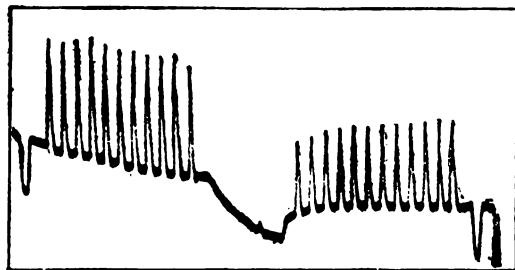
Fig. 15.—Effect of tetanisation on A (diminution) (2296).

The chief results of these experiments (and of those on temperature[*]) are to the following effect :—

The katelectrotonic current is augmented in consequence of (a) rise of temperature, (b) acidification, (c) tetanisation.

It is diminished by basification. Its augmentation by tetanisation gradually declines during repose.

The anelectrotonic current is diminished in consequence of rise of temperature. It is augmented by " very slight " acidification and by tetanisation, diminished by " slight " acidification and tetanisation.

The characteristic effect of the presumably " dissociative " influence of rise of temperature, of acidification, and of tetanisation, is a diminution of the quotient A/K. A slighter and less assured effect of tetanisation consists in an augmentation of the quotient A/K.

Note.—The foregoing observations form part of an investigation of the action of reagents on nerve, towards the expenses of which a grant was made by the Physiological Sub-committee of the British Association to Miss S. C. M. Sowton, acting as my assistant in the prosecution of the research. Our experiments during the last year have fallen under four heads :—(1) On the action of acids and alkalis upon action-currents ; (2) on the action of acids and alkalis upon electrotonic currents ; (3) on the action of temperature upon electrotonic currents ; (4) on the action of anæsthetics, of neutral salts, and of alkaloids upon electrotonic currents. The first and fourth of these four groups are not sufficiently advanced for publication, and have required to be prefaced by the second group which is now reported upon. The third group is briefly reported on in ' Roy. Soc. Proc.,' Dec. 17, 1896.

I wish to acknowledge Miss Sowton's active participation during the past year in the work above specified. Experiments under headings (1) and (4) will, it is to be hoped, be sufficiently advanced for publication during the coming year.

* ' Roy. Soc. Proc.,' vol. 60, p. 383.

(Inter- and Intra-polar Distances each of 1 cm.)

Plate No.	Time.	A.	K.	A/K.	Remarks.
2158	Normal	+ 19·5	− 8·0	6·5	Diminution of the A/K quotient by CO₂.
			CO_2		
	2—3 mins.	+ 8·0	− 3·5	2·3	
	12—18 „	+ 24·0	− 5·0	4·8	
2195	Normal	..	− 12·5	..	Primary augmentation of K by CO₂.
			CO_2		
	2 mins.	..	− 17·5		
	5 „	..	− 23·0		
	10 „	..	− 23·0		
	15 „	..	− 18·0		
	20 „	..	− 15·0		
2199 (fig. 12)	Normal	+ 7·0	..	..	Primary augmentation of A by CO₂.
			CO_2		
	8 mins.	+ 14·0			
	5 „	+ 15·0			
	10 „	+ 12·5			
	15 „	+ 9·5			
	20 „	+ 8·5			
	25 „	+ 7·5			

Table I—*continued.*

Plate No.	Time.	A.	K.	A/K.	Remarks.
2200 (fig. 18)	Normal	+21·0	..	..	Primary diminution of A by CO_2.
	CO_2 for 1 min.				
	2 mins.	+ 9·5			
	5 „	+15·0			
	10 „	+23·0			
2235	Normal	+ 7·5	− 1·5	5·0	Diminution of A/K.
	CO_2 for 1 min.				
	2—8 mins.	+ 2·5	− 1·5	1·7	
	12—18 „	+14·5	− 4·0	8·6	
2286	Normal	+ 9·5	− 4·5	2·1	
	CO_2 for 1 min.				
	10—11 mins.	+12·0	− 7·5	1·6	
2851	Normal	+19·0	− 2·5	7·6	Ill-marked effect.
	H_2SO_4, N/20				
		+18·0	− 8·0	6·0	

					Remarks
2352	Normal	+13·0	− 6·5	2·0	Marked augmentation of both A and K. A/K slightly diminished.
	Acetic acid, N/10	+21·0	−12·0	1·75	
2354	Normal	+30·0	−12·5	2·4	Augmentation of the A/K quotient by soda. Absolute diminution of A and of K.
	NaOH, N/30				
	3—4 mins.	+14·5	− 6·5	2·25	
	13—14 „	+22·0	− 4·0	5·5	
	23—24 „	+23·0	− 3·0	7·7	
	33—33 „	+22·5	− 3·0	7·5	
2355	Normal	+22·5	− 7·0	3·2	Augmentation of the A/K quotient by soda. Both A and K are absolutely diminished, but the diminution of K is relatively greater than of A. Diminution of A/K by sulphuric acid.
	NaOH, N/20				
	2—3 mins.	+14·5	− 2·0	7·2	
	H_2SO_4, N/10				
		+11·0	− 5·0	2·2	
2356	Normal	+12·0	− 2·0	6·0	Ill-marked effect.
	H_2SO_4, N/20				
	After	+10·0	− 1·5	6·7	

Table I—*continued.*

Plate No.	Time	A.	K.	A/K.	Remarks.
2357	Normal	+16·0	− 8·5	4·5	Diminution of A. Increase of K. Slight but distinct: Diminution of A/K.
	HCl, N/20				
	5—6 mins.	+14·5	− 4·0	3·8	
2358 (fig. 2)	Normal	+17·5	− 7·0	..	Absence of alteration of A and K by potash of less than optimum strength. The A/K quotient is practically constant. Alkali too weak.
	KOH, N/50				
	4—5 mins.	+18·5	− 7·5		
	34—35 ,,	+18·5	− 7·0		
2359 (fig. 3)	Normal	+21·5	− 9·0	2·4	The parallel diminution of A and of K by sulphuric acid of greater than optimum strength. The A/K quotient is not markedly altered. Acid too strong.
	H$_2$SO$_4$, N/5				
	3—4 mins.	+16·5	− 5·5	3·0	
	13—14 ,,	+13·0	− 4·0	3·25	
	23—24 ,,	+ 6·0	− 2·0	3·0	
	33—34 ,,	+ 3·5	− 1·5	2·3	

						Remarks
2360 (fig. 9)	Normal		+19·0	−8·0	2·4	Augmentation of the quotient A/K by potash in optimum concentration. K is absolutely diminished.
	3—4 mins. 33—34 „	KOH, N/20	+18·0 +15·0	−2·3 −1·5	7·8 10·0	
2363	Normal		+17·0	−12·0	1·4	Exceptional primary diminution of K by CO₂.
	2—3 mins. 12—13 „	CO₂	+2·5 +19·0	−3·5 −15·5	0·7 1·2	
2396	Normal		+15·0	−8·5	4·3	Ill-marked effect. Acid too strong.
	After	H₂SO₄, N/10	+12·0	−2·0	6·0	
2397	Normal		+21·0	−6·5	3·25	Gradual and not well-marked effect. Acid too weak.
	30—31 mins.	Formic acid, N/40	+15·0	−5·0	3·0	

Table I—continued.

Plate No	Time.	A.	K.	A/K.	Remarks.
2400	Normal	+18·0	− 5·5	3·3	Acid too strong ?
	After	+10·0	− 3·5	2·8	
	H_2SO_4, N/15				
2401	Normal	+15·5	− 5·5	2·7	Acid too strong ?
	5—6 mins.	+16·0	− 5·5	2·9	
	25—26 ,,	+12·0	− 5·0	2·4	
	H_2SO_4, N/15				
2402	Normal	+15·5	− 5·0	3·1	Diminution of A/K.
	10—11 mins.	+12·5	− 6·5	1·9	
	H_2SO_4, N/20				
2406	Normal	+22·0	− 4·5	4·9	No well-marked effect.
	4—5 mins.	+28·0	− 4·0	5·75	
	H_2SO_4, N/15				

					Diminution of A/K.
2407	Normal	+12·5	− 2·5	5·0	
	1—2 mins.	+16·0	− 4·5	3·5	H₂SO₄, N/20
2408	Normal	+13·5	− 3·0	4·5	No well-marked effect.
	5—6 mins.	+16·0	− 4·0	4·0	Formic acid, N/50
2410 (fig. 4)	Normal	+19·0	− 5·0	3·8	It is necessary to correct for altered resistance before concluding that there is any absolute alteration of A or K. In this instance the resistance was reduced by 25 per cent. by the acid, and the measurements must be corrected accordingly to + 17, − 7, + 14, − 4·5.
	3—4 mins.	+23·0	− 9·0	2·5	
	33—34 ,,	+19·0	− 6·0	3·2	Oxalic acid, N/20
2411	Normal	+20·0	− 5·5	3·6	Slight effect, but distinct.
	4—5 mins.	+20·0	− 6·5	3·0	Propionic acid, N/40

Table I—*continued.*

Plate No.	Time.	A.	K.	A/K.	Remarks.
2412 (fig. 8)	Normal	+14·0	−3·5	4·0	Diminution of the quotient A/K by propionic acid in optimum concentration. A is absolutely diminished. K is absolutely augmented.
		Propionic acid, N/10			
	5—6 mins.	+6·0	−4·5	1·3	
	30—31 ,,	+4·0	−3·0	1·3	
2413	Normal	+16·0	−5·0	3·2	Diminished A/K by relatively greater increase of K than of A.
		Propionic acid, N/20			
	3—4 mins.	+23·0	−9·0	2·5	
2422 (fig. 10)	Normal	+10·0	−1·5	7·0	Diminution of the quotient A/K by CO_2.—During passage of CO_2. A is absolutely diminished, K is absolutely augmented. Subsequently both A and K are absolutely augmented, but the augmentation of K is relatively greater than that of A, so that the quotient A/K is below normal.
		CO_2 for 8 mins.			
	1—2 mins.	+5·5	−5·5	1·0	
	7—8 ,,	+15·0	−5·0	3·0	
	11—12 ,,	+11·0	−1·5	7·7	
2426	Normal	+23·5	−6·5	3·6	Diminished A/K by relatively greater increase of K than of A.
		Formic acid, N/40			
	2—3 mins.	+26·0	−10·0	2·6	

No. (fig.)	Time				Remarks
2428—9 (fig. 6)	Normal	+ 19·0	− 6·0	3·2	Augmentation of A/K by ammonia vapour, with absolute diminution of K. Subsequent diminution of A/K by CO₂, with absolute augmentation of K, and absolute diminution of A.
			1 per cent. NH₃ vapour for 3 mins.		
	0—3 mins.	+ 19·5	− 7·0	2·8	
	4—5 ,,	+ 19·5	− 6·5	3·0	
			5 per cent. NH₃ vapour for 3 mins.		
	10—11 ,,	+ 18·5	− 6·0	3·1	
	13—16 ,,	+ 10·0	− 3·0	6·3	
	20—21 ,,	+ 16·5	− trace	—	
			CO₂ for 3 mins.		
	21—25 ,,	+ 10·0	− 7·0	1·4	
	41—42 ,,	+ 13·5	− 7·0	1·9	
	49—52 ,,	+ 17·0	− 10·0	1·7	
	53—54 ,,	+ 17·5	− 12·5	1·4	
	55—56 ,,	+ 18·5	− 10·5	1·8	
	57—58 ,,	+ 18·0	− 8·0	2·25	
	59—60 ,,	+ 17·0	− 4·5	3·8	
	61—62 ,,				
	63—64 ,,				
	69—70 ,,				
2452 (fig. 7)	1—2 mins.	+ 23·0	− 8·0	2·9	Augmentation of the A/K quotient by NH₃.
	3—4 ,,	+ 23·5	− 7·0	3·4	
	5—6 ,,	+ 24·0	− 6·0	4·0	
			5 per cent. NH₃ vapour for 4 mins.		
	7—8 ,,	+ 24·5	− 5·0	4·9	
	9—10 ,,	+ 24·0	− 3·5	6·8	
	10—20 ,,	+ 23·5	− 2·5	9·4	

Table II.—Influence of Five Minutes' Tetanisation on the A and K Currents provoked by one Leclanché Cell.

(Inter- and Intra-polar Distances each of 1 cm.)

Plate No.	Time.	A.	K.	A/K.	Remarks.
2294	Before	..	− 8·5	..	Coil at 50. Slightly augmented K.
	After	..	− 9·5		
2295 (fig. 14)	Before	+ 31·5	..	..	Coil at 50. Augmented A.
	After	+ 34·5			
2296 (fig. 15)	Before	+ 19·0	..	..	Coil at 50. Diminished A.
	After	+ 12·0			
2297	Before	..	− 7·5	..	Coil at 50. No marked effect.
	After	..	− 7·5		
2298	Before	+ 10·0	..	..	Coil at 50. Slightly diminished A.
	After	+ 8·0			
2299	Before	+ 10·0	..	..	Coil at 50. Slightly increased A.
	After	+ 11·5			
2387	Before	+ 14·5	− 5·5	2·65	Coil at 50 units. Diminished K. Augmented A/K.
	After	+ 14·5	− 4·5	3·2	
2388	Before	+ 25·5	− 9·0	2·8	Augmented A and augmented A/K. Coil at 20 units.
	After	+ 31·5	− 9·5	3·3	
2389	Before	+ 21·0	− 6·0	3·5	Augmented A and augmented K. Coil at 20.
	After	+ 25·0	− 7·0	3·5	

2390	Before	+18·0	− 8·5	3·7	Augmented A and K. Coil at 20.
	After	+16·5	− 4·5	3·7	
2391	Before	+20·0	− 7·5	3·9	Augmented A and K. Diminished A/K. Coil at 20.
	After	+32·0	−10·5	8·0	
2393	Before	+12·0	− 4·0	8·0	Augmented A and augmented A/K. Coil at 20.
	After	+14·5	− 4·0	3·6	
2394	Before	+14·5	− 6·5	2·2	Ill-marked effect. Coil at 20.
	After	+15·5	− 6·5	2·4	
2395	Before	+15·0	− 9·0	1·7	Augmented K. Diminished A/K. Coil at 20.
	After	+15·5	−11·0	1·4	
2423	Before	+19·0	− 4·5	4·2	Augmented A and K. Coil at 20.
	After	+22·0	− 5·0	4·4	
2424 (fig. 11)	Before	+15·5	− 1·0	15·5	Diminished A. Augmented K. Diminished A/K. Typical effect. Coil at 20.
	After	+11·75	− 6·5	1·8	
	15 mins. later	+14·5	− 8·0	4·8	
2425	Before	+20·0	− 4·0	5·0	Augmented K. Diminished A/K. Coil at 20.
	After	+19·5	−11·0	1·6	
2427	Before	+11·0	− 2·5	4·4	Augmented A and K. Diminished A/K. Coil at 20.
	After	+13·0	− 4·5	2·9	

"The Histology of the Cell Wall, with Special Reference to
the Mode of Connexion of Cells.   Preliminary Communica-
tion." By WALTER GARDINER, M.A., F.R.S., Fellow and
Bursar of Clare College, Cambridge.   Received August 11,
1897.

Since 1883[*] I have repeatedly endeavoured to discover some refined
and generally applicable method by means of which the fine fibrillæ,
or "connecting threads," traversing the cell membrane might be
identified with certainty, and the fact of their existence settled
beyond dispute.   I was also anxious to be in a position to investigate
the development of the threads in endosperm tissue.   My researches
met with little encouragement until 1894, when I succeeded in find-
ing a new method, by means of which I obtained excellent results
with the young developing endosperm tissue of *Tamus communis*.
This I have further elaborated, so that either the original method, or
modifications of it, can be applied to tissues generally.   In the present
communication I propose to give a brief account of my researches,
leaving a more detailed description to a future occasion.

The methods used by earlier observers for the investigation of
connecting threads are essentially based upon those of Sachs and of
Hanstein, by means of which they demonstrated the characteristic
structure of sieve-tubes.   Tangl's important results of 1880, which
were confirmed and extended by myself in 1883, were in fact
obtained by Hanstein's method as such, and are the outcome of
quite special conditions, and a happy combination of circumstances,
depending upon the fact that in dry ripe seeds the tissue is so poor in
water that with the iodine and Schulze's solution (Chlor. Zinc. Iod.),
the cellulose fails to give the usual blue colour, and thus allows the
darkly stained threads to come into view.   The method ceases to
work with unripe seeds, or even with ripe seeds which contain a
certain percentage of water, and with ordinary tissue is quite useless.

Certain modifications of the method of Sachs, and the method of
Hanstein, which may be described in general terms as involving both
a more regulated application of the swelling agents and the use of
aniline dyes in place of iodine, were first and independently intro-
duced by Russow and myself in the years 1882 and 1883.   As regards
my own researches, my first results in 1882 were obtained by a modi-
fication of Sachs' method, and consisted in swelling sections of fresh
tissue with sulphuric acid, and then staining with Hofmann's violet
(methyl violet) washed out with glycerine, or with Hofmann's blue
dissolved in picric acid (picric Hofmann's blue).   This was succeeded

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[*] Gardiner, 'Roy. Soc. Proc.,' No. 229, 1883.

in 1883 by a second method, which was a modification of that of Hanstein, and consisted in treating sections of fresh material with iodine solution, swelling with Chlor. Zinc. Iod. and staining with picric Hofmann's blue or methyl violet. Certain results obtained by this latter treatment were so promising that in my final paper in 1883, with the customary rashness of youth, I described the method as being "perfectly satisfactory;" but no long period elapsed before I found in practice that it was of but limited application.

Speaking generally, and excepting Poirault's[*] researches, I think one is justified in saying that since 1883 little or no advance has been made in the improvement of methods, and that later observations rest mainly on small modifications of the methods of Russow and myself.

The careful and detailed work of Kienitz-Gerloff[†] unfortunately serves in great measure to demonstrate the unsatisfactory nature of the results obtained by the sulphuric acid method, and to prove that unless the threads are of exceptional size, as in *Viscum album*, or as in sieve-tubes, the method is unreliable. The above remarks equally apply to such of my own results as depend upon the sulphuric acid modification.

An advance was, however, made by Poirault. In place of experimenting on sections of fresh tissue, he killed and hardened *pieces of tissue* in dilute iodine solution, and from the preserved material he then cut sections, which were swollen with Chlor. Zinc. Iod., or sulphuric acid, and stained either with eosin, Poirrier's acid brown, methyl violet, crocein, or aniline green. Poirault's researches are limited to the ferns and other vascular cryptogams, and lie buried, so to speak, in his paper "Anatomical Researches on the Vascular Cryptogams." While certain of his figures are, perhaps, not entirely convincing, the results of his research are most important, and of great interest. I am ashamed to say that I was unaware of the existence of this paper until the autumn of 1895, which was a year after I had elaborated the main lines of my own method, and applied it with success to the study of young endosperms. The great merit of Poirault's modification is that here for the first time provisions are made for preserving and hardening the tissue before taking sections. New dyes are also used. With certain kinds of tissue this method appears to have given excellent results.

I may now introduce my own researches. In the course of observations on this particular branch of cytology, certain salient facts come to the fore. In the first place one learns that material preserved in alcohol does not appear to be suitable for the investigation, and consequently fresh tissue has been used. Secondly, that it

[*] Poirault, ' Ann. Sci. Nat. (Bot.),' vol. 18, 1893.

[†] Kienitz-Gerloff, ' Bot. Zeit.,' 1891.

appears generally necessary to bring about a definite swelling of the cell wall. Thirdly, that it is not easy to stain and isolate the threads, even when they are known to be present.

These facts place many obstacles in the way of successful research. The difficulties attending the manipulation of fresh tissue are sufficiently obvious, and are apt to be so increased by the subsequent swelling as to render any really refined investigation well-nigh impossible, and as long as the threads cannot be stained so as to stand out clearly from the rest of the wall their identification is out of the question. These difficulties, which are sufficiently pronounced in the case of peculiarly favourable material such as that of endosperm, are only magnified when the investigation is concerned with ordinary or young tissue. In addition to the drawbacks already mentioned, the existing methods of research hitherto in use make no provision for the preservation of tissue.

It became obvious, therefore, that if the inquiry into the relations of the cell wall and the connecting threads was to be prosecuted with success, a more refined method must be devised, which could be reduced to terms of the usual procedure, viz., killing, fixing, hardening, preserving, cutting section, staining, and mounting, and that the methods heretofore in use were too coarse for so delicate an investigation.

I do not propose in the present paper either to give an account of the discovery of my method, or to go into elaborate technical details. It is sufficient to say that, expressed in the simplest terms, the method appears to depend upon the use of two principal reagents, viz., the osmic-acid-uranium-nitrate mixture of Kolossow as a fixative, and safranin as a dye. As a preservative I have used thymol water, and have obtained excellent results with material which has remained in it for as long a period as three years. Sections may be cut by hand or with the freezing microtome.

The fixing and staining reagents must be introduced and employed in different ways, the exact manner of procedure depending upon the character and age of the particular tissue under observation. This can be best illustrated by means of definite examples, and since the whole method is somewhat complicated, it will be expedient to consider under separate heads (1) the killing and fixing, with which is also associated the swelling, and (2) the staining.

In material such as that of young endosperms (*e.g.*, the endosperm of *Tamus communis*), no swelling is required, and the tissue, cut into small pieces, may be both killed and fixed at one and the same time by Kolossow's reagent, and then preserved in thymol water for future use. Where only slight swelling is necessary, treatment with water may precede that of Kolossow's reagent. In certain classes of tissue, where the walls are swollen with comparative ease, such as

that of the ordinary vegetative tissue of *Phaseolus vulgaris, Tamus communis, Nerium oleander, Salisburia adiantifolia,* &c., small pieces are killed and swollen in an aqueous solution of picric acid, and then fixed in the Kolossow's reagent and preserved in thymol water. Finally, where the tissues are more resistant, as, for instance, in *Robinia pseudacacia, Prunus laurocerasus,* or *Aucuba japonica,* treatment with picric acid may be followed by more severe swelling by means of solutions of zinc chloride or sulphuric acid, to be succeeded as before by fixing, hardening, and preserving. The blackening of the cell contents caused by osmic acid may be removed by bleaching.

From such preserved material sections may be cut when required.

The process of staining is no less complicated than that of killing and fixing, and is best considered under two heads, viz. :—(1) The methods applicable to certain endosperms and tissues of similar character. (2) The methods applicable to the majority of tissues.

In certain special cases it is possible to stain the threads directly either with safranin alone or by introducing safranin by means of a somewhat intricate substitution method such as that which I used with excellent results in the case of the endosperm of *Tamus communis,* where the sequence of staining was Hofmann's blue (or soluble water blue), methylene blue, safranin, and in which moreover the Hofmann's blue was dissolved in dilute picric acid or uranium nitrate, and the methylene blue in dilute salt solution. Once stained with safranin, all sorts of modifications are possible. Thus, the safranin may be succeeded by gentian violet or by eosin, and with gentian violet Gram's method is applicable and most advantageous. As safranin forms a precipitate with chromic acid, sections stained with safranin may be treated with this reagent, and then with silver nitrate, thus effecting a silver staining of the threads. Silver nitrate itself also forms a precipitate with safranin. In a.. cases the staining is practically limited to the threads.

When the above methods of direct selective staining are applied to ordinary tissue they are found to fail, for it usually happens that the whole of the wall becomes deeply stained, so that the threads are no longer visible. I was for some time completely baffled by this circumstance, but I ultimately adopted the well-known method of staining and washing out, using for the purpose orange G or acid fuchsin. With ordinary tissue the staining appears to be more easily accomplished than with the thick mucilaginous walls of endosperm cells, and the method may be somewhat more syncopated. Excellent results may be obtained by staining at once with safranin and washing out with orange G. This may be followed by staining with gentian violet, succeeded by treatment with acid fuchsin, or the sequence of staining may be safranin, gentian violet, acid fuchsin. Substitutions in which safranin, gentian violet, and eosin are included

give good results. The method of staining indirectly by washing out may also be applied to endosperm tissues generally. The stained sections are best examined either in water or in very dilute glycerine. I have as yet given little attention to the question of making permanent preparations, although I have initiated certain experiments which may possibly lead to a satisfactory result.

It is probable that the method in its present form will not be found to be available for the study of adult lignified or suberised cells, though up to the present I have made no observations upon such tissues. The investigation of young tissue will, however, doubtless give good results, and will establish that in them, also, the general structure prevails. I am strongly convinced that the above method, or similar methods, based upon "non-alcoholic" treatment, will be found to be peculiarly suitable for the investigation of the tissues of plants, and will in the future lead to observations of interest and importance.

A summary of results may now follow.

As my new method owes its origin to a study of the cells of endosperm I propose to deal with that tissue first.

The present investigation entirely confirms and extends the results I obtained in 1883.[*] At that time, however, many important problems still awaited solution. In the first place, what was believed to be the typical and universal structure could only be demonstrated with particularly favourable material and in a limited number of plants. Further, even in ripe seeds, where treatment with iodine proved that connecting threads were present, special difficulties were experienced when attempts were made to stain them with aniline dyes. Lastly, for young seeds the methods were quite unsuitable. With the present methods we are in a position to investigate the structure of endosperms generally, and to follow their development even from the earliest stages. The more refined method also gives more sure and satisfactory results.

It is possible to make certain general statements concerning the connecting threads of endosperm cells. In the first place, the histological structure of endosperm establishes a point of great importance which is only emphasised by the study of tissues generally, viz., that in pitted cells the pit-closing membrane is invariably traversed by threads. For descriptive purposes these may be called "pit-threads." Threads may also be present which traverse the general wall, and these may, similarly, be called "wall-threads." In the somewhat exceptional cases of unpitted cells the thread system is necessarily composed of "wall-threads" only. In many pitted cells both "pit-threads" and "wall-threads" are present; but in the majority of cells the threads appear to be limited to the pits,

[*] Gardiner, 'Roy. Soc. Proc.,' No. 225, 1883.

though it is not improbable that even in such cases other threads traversing the general wall will be found to occur.

Where both " pit-threads " and " wall-threads " coexist in one and the same cell, the former are stouter and more readily stainable than the latter. In pitted cells the pit-threads are necessarily in groups, and it is a point of some interest that the wall-threads also are usually in groups—as though a pit were present. This is especially striking in such cases as the unpitted cells of *Tamus communis* (fig. 1) and *Hordeum vulgare*. In *Tamus communis*, while the side walls exhibit the usual arrangement of isolated groups of threads, the end walls are traversed by a single large group, as in sieve-tubes. The structure of the endosperm cells of *Lilium martagon* (fig. 2) is

FIG. 1.
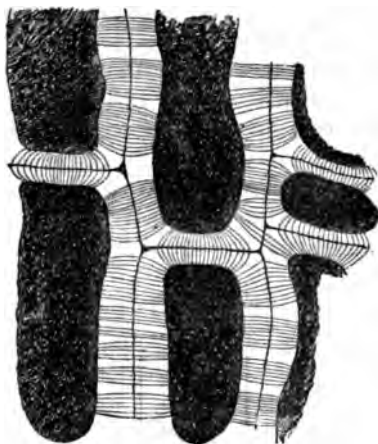
FIG. 2.
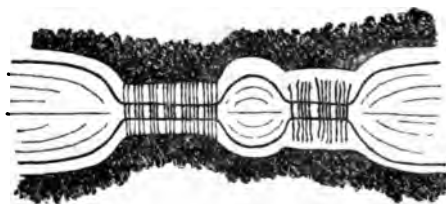
of some interest. The cells are pitted and each pit has its group of " pit-threads." In any given group the threads are arranged in bundles, recalling the similar arrangement of achromatin fibres, which Strasburger and others have found to accompany nuclear divisions in this plant. The mode of development of the threads

and the phenomena which accompany germination were investigated in the endosperm of *Tamus communis* only. The threads are found to be present at a very early stage, and can be detected even in the very youngest and thinnest walls. They are at first uniformly distributed over the cell membrane. In the case of the side walls, as surface growth proceeds, small groups of threads become separated from one another by intervening areas of clear membrane, while in the end walls, where the extension of surface is less, this segregation into groups does not take place. In the early stages the growth in thickness of the wall is not uniform, and pits are formed on the side walls at those points where thread groups occur; but they have only a transient existence, and ultimately disappear. It is, however, interesting to note that the vegetative tissue of *Tamus communis* consists for the most part of pitted cells.

In germination the ferment, in the first instance, appears to be conducted into the wall by means of certain of the threads, but when once an entrance is effected the corrosive action rapidly spreads quite independently of the threads, becoming the more potent as it reaches the neighbourhood of mucilaginous and less resistent middle lamella. In a given wall the penetration commences simultaneously at several centres, and at each centre the affected areas assume the general form of small cones with their apices directed towards the cell lumen (fig. 3). Moreover, since the action of the

FIG. 3.

ferment soon extends from cell to cell in two adjoining cells, where the common wall is affected on both sides, each side having its cone, the base of one cone appears opposed to that of the other. At this stage, by appropriate staining, the threads may still be seen shining through the disorganised mucilage of the affected areas. As the ferment action proceeds the boundaries of the several areas continually extend and at length unite when the whole of the wall is involved. The disorganisation of the wall is accompanied by marked stratification. The sphere of influence of the ferment action

is curiously limited and hardly extends beyond the immediate neigh-
bourhood of the absorbent foot of the young embryo. The mode of
disappearance of the wall by the coarse corrosive action of the
ferment, and the manner of its proceeding with so little relation to
the threads, seems to indicate that the structure of endosperm cells
has more relation to the conduction of impulses and of food supply
than to the needs of germination.

Satisfactory as are the results derived from the study of endo-
sperm cells, the real success was achieved when the method was so
modified that it could be applied with certainty and success to the
investigation of ordinary tissue, and to specialised forms of it, such
as pulvini and tendrils.

The unsatisfactory character of the somewhat meagre results
hitherto obtainable have not, I think, succeeded in generally estab-
lishing a conviction that the structures postulated have necessarily
an actual existence. Botanists have, on the contrary, been inclined
to look askance at results depending upon such statements as "a
stained area" or "a doubtful striation," and there lurked in the
minds of many, the suspicion that the histology of endosperms was
possibly exceptional, and peculiar to that tissue.

For the future such doubts need no longer be entertained, since
it is now easy to demonstrate that the structure exhibited by
endosperm tissue is in all respects entirely typical of plant tissue
generally.

In these days of active investigation, it is not often given to one to
be the medium for the criticism of his own research. This good
fortune, however, now falls to me, and I hasten to say that in the
light of the present investigation it is quite clear that apart from
endosperms, and with such exceptions as *Aucuba japonica*, my earlier
work on the continuity of the protoplasm, in pulvini and other
tissues, does not afford absolute proof that a communication between
cells actually occurs, but for the most part only brings forward
strong evidence that such connection is exceedingly probable. At
this juncture, also, I note. with satisfaction, that the results then
obtained were able to save me from the error of a belief in the
existence of a system of open pits which have since then been
repeatedly figured and described, and against which I have persist-
ently spoken.

As I have already remarked, my new method makes it possible to
establish with certainty that the structure of all kinds of plant
tissue is precisely similar to that of endosperms, and that exactly
the same modifications are exhibited. In pitted cells every pit-
closing-membrane is traversed by its group of threads, and in un-
pitted cells similar groups also occur. In square-ended tubular cells,
such as those of the leaf stalk of *Lilium martagon* (fig. 4), the

FIG. 4.
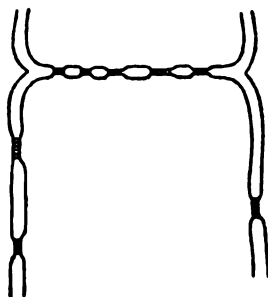
numerous small isolated groups of wall-threads are present on the side walls, and a large group occupies each end wall. In certain cells both "pit-threads" and "wall-threads" may simultaneously be present. The method gives equally good results with thin or thick-walled tissue, and in the case of the thinnest walls an "*en face*" view can be obtained where a sectional view fails. The threads vary in size. They are, for instance, exceedingly thick in *Viscum album*, where they are seen with the greatest ease; they are well developed in *Phaseolus multiflorus* and *Lilium martagon*, and they are fine and delicate in *Aucuba japonica* (fig. 5). The connecting threads may be

FIG. 5.
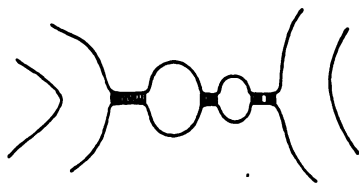
observed equally in the epidermis and cortex, and in all the living cells of the tissues of the central cylinder. In a section of any given tissue, it can be readily seen that all the cells are placed in communication with one another. One is tempted to expatiate upon the wonderful beauty of the appearance presented by the walls studded with their pits, each pit with its closing membrane traversed by the thread complex after the manner of sieve-tubes. It is indeed a sight which cannot fail to convey a lasting impression of wonder and surprise.

The pit-threads of such pulvini as were examined presented no striking difference, either in appearance or distribution, to the similar structures in ordinary tissue, and the same appears to be true of the sieves of tendrils. The important point, however, is this,

that in all these tissues the threads perforating the pit-closing membrane can be readily seen, and even counted.

A point of interest was observed in the epidermal cells of *Tamus communis* and *Lilium martagon* (fig. 6), viz., that the external or free walls are penetrated by a system of threads which radiate from the cell lumen and extend to the cuticle, so that the latter is the only structure intercalated between the protoplasm and the environment. The important bearings of this observation are obvious.

FIG. 6.
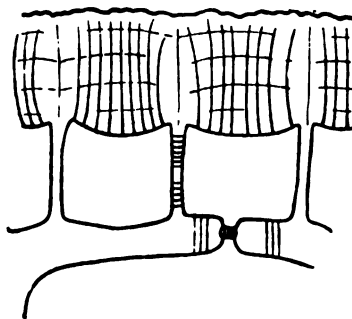
The following is a list of the tissues which have up to the present been investigated, together with the date of observation. In each case connecting threads were observed.

In 1894. The young and developing endosperm of *Tamus communis* and the young endosperm of *Asperula odorata.*

In 1895. The cotyledons of *Tropæolum majus.* The endosperm of *Lilium martagon* and *Fritillaria imperialis.* The root of *Ranunculus asiaticus.* The leaf stalks of *Tamus communis, Viscum album,* and *Marattia elegans.*

In 1897. The leaf stalks of *Aucuba japonica, Prunus laurocerasus, Nerium oleander, Lilium martagon, Lilium candidum, Salisburia adiantifolia,* and *Asplenium rutæfolium* (fig. 7). The flower stalk of

FIG. 7.
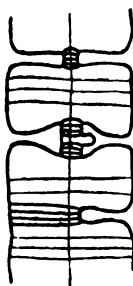
Taraxacum dens-leonis. The perianth-leaves of *Lilium martagon.* The pulvini of *Mimosa sensitiva* and *Robinia pseudacacia* (fig. 8). The tendril of *Cucurbita pepo.* The endosperm of *Hordeum vulgare.*

FIG. 8.
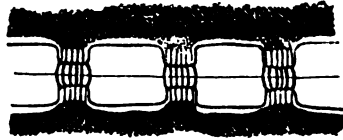
Although the research is still in its preliminary stages, since my attention has been chiefly directed to the elaboration of the new method, yet the results already obtained are sufficiently numerous and suggestive to enable one to make certain remarks and general statements.

It is impossible to resist the conclusion that the connecting threads consist either wholly or partly of protoplasm, and this view is largely confirmed by the staining reactions. It is, however, not improbable that the protoplasmic filament may be surrounded by a mucilaginous sheath. Osmic acid induces no blackening as it does in the threads of many sieve-tubes, and I am inclined to believe that in ordinary tissue the threads consist for the most part of ectoplasm, and are to be regarded in fact as extensions of it.

The threads appear to be present *ab initio.* This fact, coupled with the surface-growth of the cell wall, furnishes a sufficient explanation of the "barrel figures" so generally assumed by the various thread groups. The resemblance to the similar figures which accompany nuclear division is therefore superficial. Nevertheless it seems certain that the threads do, as a matter of fact, arise from that part of the cytoplasm which at the period of formation of the cell plate forms the fibres of the so-called "nuclear spindle," and that these fibres become, so to speak, partially imprisoned in the young wall.

My results appear to indicate that in a given cell the whole system of connecting threads arise at this early stage, and that no subsequent development occurs. This statement will, however, require careful confirmation, and has certain bearings on such interesting questions as the theory of grafts and "sliding growth."

In dead cells, such as those of ripe endosperm, the threads appear to degenerate into mucilage, and this is possibly also the case in adult lignified and other cells.

In the particular tissues which I have investigated, the threads can be shown to be present in all cells which still retain their cellulose character, and although I have not actually succeeded in

observing them in adult lignified and suberised tissue, it seems
certain that they will be found there also. There can be little doubt
that they occur universally in the cells of all the tissues of all
plants.

From this arises the fundamental conception that the plant body
must be regarded as a connected whole, and that the cell walls
occupy only a subservient position. Thus our views as to the ulti-
mate histology of tissue must be considerably modified. A new vista
also opens to cytological research in the direction of the accurate
determination of the distribution and orientation of the threads in the
various tissues, which can hardly fail to lead to important results.

Should the structure presented by the external walls of the epi-
dermal cells of *Tamus communis* and *Lilium martagon* be found to be
of general occurrence, we shall be prepared for most interesting
results when the examination is extended to secreting gland cells:
to such non-cellular organisms as certain algæ and fungi, and to such
unicellular bodies as spores and pollen grains.

Two important functions are, doubtless, performed by the con-
necting threads, viz., the conduction of impulses and the conduc-
tion of food. As to the first, there can be no question; and
as to the second, one cannot but reflect that it must be of the
greatest advantage to the plant to be able to transmit from cell to
cell as occasion requires, and in a definite and determinate direction,
highly organised food supplies and even protoplasm itself. It is, of
course, possible that in the threads themselves a definite division of
labour may occur as regards the transmission of food and the con-
duction of stimuli.

The consequences which arise from our more perfect knowledge of
plant histology are obvious and far reaching.

In the first place we learn that the structure exhibited by sieve-
tubes, which in the past was regarded as peculiar, is shown to
be typical of cells generally, with this slight difference, however,
that in sieve-tubes a secondary enlargement of the pores appears to
occur. All the cells of a tissue must be regarded as being connected
together by delicate groups of protoplasmic threads, which traverse
either the general wall or the pit-closing membrane, just as the
sieve-tube threads traverse the so-called "sieve plate," a fact which
at length enables us to do tardy justice to the dominant position
occupied by the protoplasm of the plant body, and to understand
how the deep-seated cells of a tissue can telegraph their needs to
those at the periphery, cell after cell taking note of the wording
of the message, or how the peripheral cells may communicate to the
interior their sense of gravity, light, heat, or touch, to which the
whole organ may reply as its peculiar organisation directs.

As an integral part of cell structure, the connecting threads con-

stitute a factor, which cannot fail to have an important bearing in all general questions, such as the growth of the cell wall, the conduction of food, the ascent of water, the process of fertilisation, the penetration of fungi into their host, the process of secretion, and the transmission of the impulses which determine growth and movement of plant organs.

Concerning certain of these problems, I should like to make a few concluding remarks.

As to the passage of water from the root hair to the vessel, the presence of connecting threads in the cells of root tissue makes it possible to imagine that the ordinary laws of osmosis may be profoundly modified, and that the filaments which establish protoplasmic continuity may conduct stimuli, leading, for instance, to a difference in reaction of the proximal and distal halves of any given cell. Similarly, it is conceivable that a definite polarity is established, which helps to determine the direction of the flow. As to the larger question of rapid water movement, although this is neither the time nor the place to enter into theory, yet I cannot refrain from remarking that it is not impossible that the threads, doubtless present in large quantities in dead vessels, may, if they suffer mucilaginous change, have some bearing on the question, *e.g.*, by assisting to sustain the water at any given level or attracting water in the immediate environment. In any case, I am strongly of opinion that the part played by mucilage and the force of hydration have not as yet received sufficient attention.

As to movements generally, I am still unable to accept Pfeffer's view of the subsidiary part played by the protoplasm in connexion with turgidity,[*] and I am still of opinion that the ectoplasm is the master factor which determines the condition of the cell. The present research demonstrates among other things that there are fixed points in the ectoplasm, and this may have some bearing on the possibility of establishing the periodic or sudden contractions and dilatations which I believe are associated with turgescence, and of which such a phenomenon as the effusion of water from the cells of a stimulated *Mimosa* pulvinus is but an abnormal instance.

"On the Viscosity of Hydrogen as affected by Moisture." By LORD RAYLEIGH, F.R.S. Received September 8, 1897.

In Sir W. Crookes's important work upon the viscosity of gases[†] the case of hydrogen was found to present peculiar difficulty. " With each improvement in purification and drying I have obtained

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* Gardiner, ' Roy. Soc. Proc.,' vol. 43, 1887.

† ' Phil. Trans.,' 1881, p. 387.

a lower value for hydrogen, and have consequently diminished the number expressing the ratio of the viscosity of hydrogen to that of air. In 1876 I found the ratio to be 0·508. In 1877 I reduced this ratio to 0·462. Last year, with improved apparatus, I obtained the ratio 0·458, and 1 have now got it as low as 0·4439 " (p. 425). The difficulty was attributed to moisture. Thus (p. 422): "After working at the subject for more than a year, it was discovered that the discrepancy arose from a trace of water obstinately held by the hydrogen—an impurity which behaved as I explain farther on in the case of air and water vapour."

When occupied in 1888 with the density of hydrogen, I thought that viscosity might serve as a useful test of purity, and I set up an apparatus somewhat on the lines of Sir W. Crookes. A light mirror, 18 mm. in diameter, was hung by a fine fibre (of quartz I believe) about 60 cm. long. A small attached magnet gave the means of starting the vibrations whose subsidence was to be observed. The viscosity chamber was of glass, and carried tubes sealed to it above and below. The window, through which the light passed to and fro, was of thick plate glass cemented to a ground face. This arrangement has great optical advantages, and though unsuitable for experiments involving high exhaustions, appeared to be satisfactory for the purpose in hand, viz., the comparison of various samples of hydrogen at atmospheric pressure. The Töppler pump, as well as the gas generating apparatus and purifying tubes, were connected by sealing. But I was not able to establish any sensible differences among the various samples of hydrogen experimented upon at that time.

In view of the importance of the question, I have lately resumed these experiments. If hydrogen, carefully prepared and desiccated in the ordinary way, is liable to possess a viscosity of 10 per cent. in excess, a similar uncertainty in less degree may affect the density. I must confess that I was sceptical as to the large effect attributed to water vapour in gas which had passed over phosphoric anhydride. Sir W. Crookes himself described an experiment (p. 428) from which it appeared that a residue of water vapour in his apparatus indicated the viscosity due to hydrogen, and, without deciding between them, he offered two alternative explanations. Either the viscosity of water vapour is really the same as that of hydrogen, or under the action of the falling mercury in the Sprengel pump decomposition occurred with absorption of oxygen, so that the residual gas was actually hydrogen. It does not appear that the latter explanation can be accepted, at any rate as regards the earlier stages of the exhaustion, when a rapid current of aqueous vapour must set in the direction of the pump; but if we adopt the former, how comes it that small traces of water vapour have so much effect upon the viscosity of hydrogen?

It is a fact, as was found many years ago by Kundt and Warburg[*] (and as I have confirmed), that the viscosity of aqueous vapour is but little greater than that of hydrogen. The numbers (relatively to air) given by them are 0·5256 and 0·488. It is difficult to believe that small traces of a foreign gas having a 6 per cent. greater viscosity could produce an effect reaching to 10 per cent.

In the recent experiments the hydrogen was prepared from amalgamated zinc and sulphuric acid in a closed generator constituting in fact a Smee cell, and it could be liberated at any desired rate by closing the circuit externally through a wire resistance. The generating vessel was so arranged as to admit of exhaustion, and the materials did not need to be removed during the whole course of the experiments. The gas entered the viscosity chamber from below, and could be made to pass out above through the upper tube (which served also to contain the fibre) into the pump head of the Töppler. By suitable taps the viscosity chamber could be isolated, when observations were to be commenced.

The vibrations were started by a kind of galvanometer coil in connection (through a key) with a Leclanché cell. As a sample set of observations the following relating to hydrogen at atmospheric pressure and at 58° F., which had been purified by passage over fragments of sulphur and solid soda (without phosphoric anhydride), may be given:—

Observations on June 7, 1897.

—	65·4	—	—	—
423·7	88·9	358·3	2·554	—
401·3	110·0	312·4	2·495	0·059
381·5	128·9	271·5	2·434	0 061
364·4	144·1	235·5	2·372	0·062
349·7	158·6	205·6	2·313	0·059
336·8	169·8	178·2	2·251	0·062
325·7	180·6	155·9	2·193	0·058
315·7	189·8	135·1	2·131	0·062
307·2	197·8	117·4	2·070	0·061
300·0	204·6	102·2	2·009	0·061
293·7	210·6	89·1	1·950	0·059
287·8	—	77 2	1·888	0·062

Mean log. dec. = 0·0604.

The two first columns contain the actually observed elongations upon the two sides. They require no correction, since the scale was bent to a circular arc centred at the mirror. The third column

* 'Pogg. Ann.,' 1875, vol. 155, p. 547.

gives the actual arcs of vibration, the fourth their (common) logarithms, and the fifth the differences of these, which should be constant. The mean logarithmic decrement can be obtained from the first and last arcs only, but the intermediate values are useful as a check. The time of (complete) vibration was determined occasionally. It was constant, whether hydrogen or air occupied the chamber, at 26·2 seconds.

The observations extended themselves over two months, and it would be tedious to give the results in any detail. One of the points to which I attached importance was a comparison between hydrogen as it issued from the generator without any desiccation whatever and hydrogen carefully dried by passage through a long tube packed with phosphoric anhydride. The difference proved itself to be comparatively trifling. For the wet hydrogen there were obtained on May 10, 11, such log. decs. as 0·0594, 0·0590, 0·0591, or as a mean 0·0592. The dried hydrogen, on the other hand, gave 0·0588, 0·0586, 0·0584, 0·0590 on various repetitions with renewed supplies of gas, or as a mean 0·0587, about 1 per cent. smaller than for the wet hydrogen. It appeared that the dry hydrogen might stand for several days in the viscosity chamber without alteration of logarithmic decrement. It should be mentioned that the apparatus was set up underground, and that the changes of temperature were usually small enough to be disregarded.

In the next experiments the phosphoric tube was replaced by others containing sulphur (with the view of removing mercury vapour) and solid soda. Numbers were obtained on different days such as 0·0591, 0·0586, 0·0588, 0·0587, mean 0·0588, showing that the desiccation by soda was practically as efficient as that by phosphoric anhydride.

At this stage the apparatus was rearranged. As shown by observations upon air (at 10 cm. residual pressure), the logarithmic decrements were increased, probably owing to a slight displacement of the mirror relatively to the containing walls of the chamber. The sulphur and soda tubes were retained, but with the addition of one of hard glass containing turnings of magnesium. Before the magnesium was heated the mean number for hydrogen (always at atmospheric pressure) was 0·0600. The heating of the magnesium to redness, which it was supposed might remove residual water, had the effect of *increasing* the viscosity of the gas, especially at first.[*] After a few operations the logarithmic decrement from gas which had passed over the hot magnesium seemed to settle itself at 0·0606. When the magnesium was allowed to remain cold, fresh fillings gave again 0·0602, 0·0601, 0·0598, mean 0·0600. Dried air at 10 cm.

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[*] The glass was somewhat attacked, and it is supposed that silicon compounds may have contaminated the hydrogen.

residual pressure gave 0·01114, 0·01122, 0·01118, 0·01126, 0·01120, mean 0·01120.

In the next experiments a phosphoric tube was added about 60 cm. long and closely packed with fresh material. The viscosity appeared to be slightly increased, but hardly more than would be accounted for by an accidental rise of temperature. The mean uncorrected number may be taken as 0·0603.

The evidence from these experiments tends to show that residual moisture is without appreciable influence upon the viscosity of hydrogen ; so much so that, were there no other evidence, this conclusion would appear to me to be sufficiently established. It remains barely possible that the best desiccation to which I could attain was still inadequate, and that absolutely dry hydrogen would exhibit a less viscosity. It must be admitted that an apparatus containing cemented joints and greased stop-cocks is in some respects at a disadvantage. Moreover, it should be noticed that the ratio 0·0600 : 0·1120, viz. 0·536, for the viscosities of hydrogen and air is decidedly higher than that (0·500) deduced by Sir G. Stokes from Crookes's observations. According to the theory of the former, a fair comparison may be made by taking, as above, the logarithmic decrements for hydrogen at atmospheric pressure, and for air at a pressure of 10 cm. of mercury. I may mention that moderate rarefactions, down say to a residual pressure of 5 cm., had no influence on the logarithmic decrement observed with hydrogen.

I am not able to explain the discrepancy in the ratios thus exhibited. A viscous quality in the suspension, leading to a subsidence of vibrations independent of the gaseous atmosphere, would tend to diminish the apparent differences between various kinds of gas, but I can hardly regard this cause as operative in my experiments. For actual comparisons of widely differing viscosities I should prefer an apparatus designed on Maxwell's principle, in which the gas subjected to shearing should form a comparatively thin layer bounded on one side by a moving plane and on the other by a fixed plane.

"On the Variation of the Electromotive Force of different Forms of the Clark Standard Cell with Temperature and with Strength of Solution." By H. L. CALLENDAR, M.A., F.R.S., McDonald Professor of Physics, and H. T. BARNES, M.A.Sc., Demonstrator of Physics, McGill University, Montreal. Received August 12, 1897.

§ 1. *Objects of the Investigation.*

The primary object of the present series of experiments was that of equipping the McDonald Physics Building of McGill University with a reliable and accurate set of standard cells, and not that of forming the subject of a communication to any scientific paper. In the course of the work, however, several points have come under our notice, which we venture to think may be of interest to others engaged in any investigation requiring the employment or construction of such standards.

Among other points, we have devoted special attention to the accurate determination of the temperature-coefficients of various forms of Clark cell; to the construction of cells free from "diffusion-lag" consequent upon change of temperature; and to the investigation of the limits of accuracy attainable with Clark cells under both constant and varying temperature conditions.

We have succeeded in making a very simple modification in the Board of Trade form of Clark cell, which makes it equal to any other form in respect of freedom from diffusion-lag; and we have made several forms of cell hermetically sealed with glass and platinum, which we hope will stand the test of time better than those sealed with wax and marine glue.

We have also made a special investigation of the effect of changes of strength of the solution of zinc sulphate on the E.M.F., involving determinations of the solubility of zinc sulphate and of the density of the solutions, which appear to lead to very simple formulæ of some theoretical interest.

§ 2. *Preliminary Work.*

At the outset of our work, we were extremely fortunate in finding the laboratory already equipped by the liberality of Mr. W. C. McDonald, the donor of the Physics Building, with a very fine and complete set of resistance standards and electrical measuring instruments, collected by Professor John Cox, under whose supervision the laboratory was planned and erected. There were in the collection several portable Clark cells by Muirhead, and a set of Carhart cells by Queen and Co. were shortly added.

K 2

Several comparisons of these portable cells were made early in 1894 at different dates and under ordinary laboratory conditions. The results, when corrected for temperature by means of the enclosed thermometers, showed irregular differences, often amounting to nearly 3 millivolts. Of a pair of cells enclosed in one brass case together with a thermometer, one would be sometimes higher and sometimes lower than the other, although the utmost care was observed in using them, and they were both necessarily always under closely similar conditions as regards change of temperature.

The set of Carhart cells were much better in this respect. They rarely showed irregular differences of more than half a millivolt; but one of the cells, owing to defective sealing, was generally some 2 millivolts lower than the others, and has since that time fallen still further. It appeared probable at first that these discrepancies were partly due to inequalities of temperature between the cells and their attached thermometers, and, if so, that they would be inseparable from portable cells of such a form under these conditions. About the same time, a number of Clark cells were set up by Professor Callendar and some of the advanced students and demonstrators, in accordance with the form described in the Board of Trade Memorandum, as figured in Glazebrook and Shaw's 'Practical Physics,' p. 576. Every detail of manipulation and construction was carefully followed, except that the cells were set up in test-tubes 6 inches long, to permit of their being more deeply immersed in water, and that the glass tubes containing the electrodes were provided with mercury cups at the top to facilitate the making and changing of connections. The cells were kept immersed in water in large glass bottles provided with a stirrer and thermometer.

The cells set up in this way, although made by different students with different solutions at different dates, were found to agree more closely among themselves than the portable forms, owing probably to the more constant and certain conditions to which they were exposed. They still, however, exhibited irregular differences, even when exposed to precisely similar variations of temperature, and it was felt that they could not be used with confidence for any work in which an accuracy of one part in 10,000 was desired.

Some determinations were also made of the temperature coefficients of these cells when exposed to a variation of temperature at the rate of about 10° in two hours. The results were very fairly consistent for each cell, but gave very different values for the mean coefficient between 10° and 20° C. The lowest value obtained was 0·00045, the highest 0·00069. The value commonly given for these cells is 0·00078. They were all saturated cells containing an abundance of crystals, which remained visible on the top of the paste and throughout the mass at the highest temperature.

In examining the results it was noticed that the value of the co-efficient did not depend on the quantity of crystals in the cell, but on the distance of the end of the zinc rod from the crystals. The difference of the results was, therefore, evidently due to slowness of diffusion of the state of saturation through the body of the solution.

So long ago as 1886 a set of five cells was set up by one of the present writers at the Cavendish Laboratory, Cambridge, with zinc rods of different lengths, with the object of testing the effect of diffusion on the temperature-coefficient. These five cells were of the original Rayleigh pattern, with platinum wires sealed through the bottom. They could not be immersed in a water-bath, and were found to be of a somewhat unsuitable form for the experiment, which was then discontinued for more important work. Some more recent tests of this set of cells will be found in the paper on the Clark cell, by Glazebrook and Skinner.* The cells are numbered, 6, 7, 8, 9, and 10 in the paper, and are among the oldest in the possession of the Cavendish Laboratory. It will be seen from the tests that they exhibit small discrepancies such as might very probably arise from differences in the time required for diffusion in the different cells. From a study of the above-mentioned paper, it appears likely, in our opinion, that the differences observed in the case of the other cells of the same type may have been affected by a similar cause.

It would appear, in fact, inevitable that cells of the simple Board of Trade pattern should exhibit some effects of diffusion-lag, especially if subjected to considerable or rapid changes of temperature. This form of cell, however, is so convenient to use, and so easy to make, that it seemed to us desirable to make a more careful examination of the case, with the object, if possible, of constructing cells of this form with a definite temperature-coefficient and a negligible diffusion-lag.

In October, 1894, the class was joined by Mr. H. T. Barnes, who, as assistant to Dr. Harrington, Professor of Chemistry, had obtained considerable experience in chemical manipulation. From this date onwards the work of making and testing various forms of Clark cell has been performed almost entirely by Mr. Barnes, but the observations and calculations throughout have been checked and verified by Professor Callendar, who has devoted special attention to the thermometry.

§ 3. *Constant-temperature Baths.*

The first step in the investigation consisted in making a pair of suitable water-baths, controlled by delicate thermostats, which could

* 'Phil. Trans.,' A, vol. 183 (1892), p. 586.

be set in such a manner as to keep the temperature steady for any desired length of time at any point between 5° and 30° C.

The method of regulation adopted for these baths was similar to that described by Griffiths,* but much less elaborate. The baths were made of copper, and were encased in felt and wood. They were heated by a stream of tap-water passing through a copper tube over a regulated gas flame. The regulators were made to cut off the gas so sharply that a difference of temperature of one-tenth of a degree sufficed to change the gas supply from full flame to no flame. With these very sensitive regulators, some trouble was experienced at first, partly owing to the excessive variations of the Montreal gas pressure, and partly owing to the sudden changes of the climate. In the end, however, these difficulties were so successfully overcome that, on the longest continuous run, extending over nearly a fortnight, the temperature of the bath did not vary by so much as 0·02° C. throughout the whole period.

The temperature of the tap-water averaged about 8° C. in mid-winter, and seldom rose above 13° C. in summer. This generally sufficed to keep the baths steadily at 15° C. even if the temperature of the room was as high as 25° C. In order to obtain steady temperatures at points below 15° C., the stream of tap-water was led through a copper spiral immersed in melting snow or ice before passing over the gas flame. In this manner the baths could be set to regulate steadily at temperatures as low as 5° C.

The two baths were generally set to regulate at different temperatures, so that by transferring a cell from one bath to the other the effect of a sudden and definite change of temperature could be observed. The time required by cells of different forms to reach their steady final values could thus be determined, and the effects of diffusion-lag could be readily distinguished from the immediate change of E.M.F. consequent upon a change of temperature.

§ 4. *Electrical Apparatus.*

By the use of these accurately regulated water-baths, the temperature of the cells became so much a matter of certainty that we found it desirable to make the comparisons to the hundredth of a millivolt, corresponding to the hundredth of a degree Centigrade of temperature. From the results of our experiments, we have reason to conclude that the Clark cell, under suitable conditions, permits the attainment of this order of accuracy, and is far superior to the silver voltameter for accurate determinations.

The comparisons of the cells were made by the usual Poggendorff method, with a 6000-ohm galvanometer. The potentiometer used

* ' Phil. Trans.,' A, vol. 184 (1893), p. 374.

for the earlier comparisons was a long wire, having a resistance of 86 ohms, wound on a cylinder in one hundred turns. Each turn was divided into one hundred parts, and readings were taken to one tenth of a division. Each division corresponded approximately to one five-thousandth part of a volt, a storage-cell being used to supply the steady current through the wire. This potentiometer had been accurately calibrated throughout its length at two different dates. The results agreed so closely that it could be used with confidence for measuring relatively large differences of potential with an accuracy of at least one-half division of the wire, equivalent to 0·0001 of a volt. The errors of the uncorrected wire amounted to over ten divisions in many places.

For the later experiments a simpler, and in many respects more convenient, form of potentiometer was used. Two resistance boxes, containing resistances adjustable up to 2000 ohms each, were connected by a platinum-silver bridge-wire having a resistance of 18 ohms. The wire was 2 metres long, in four lengths of 50 cm. each, with a millimetre scale, and was adjusted to read direct in volts, at the rate of 1 mm. to one-hundredth of a millivolt, in the following manner:—A resistance of 18/20 × 1420 ohms was taken out of the first box, and the resistance in the second box was adjusted to make the standard cell at 15° C. read near the point 140 cm. of the wire, *i.e.*, 14 millivolts above 1·420 volts. The bridge-wire could thus be used directly for measuring small differences of E.M.F. not exceeding 20 millivolts, with an accuracy of at least one part in a thousand on a difference of this order. Larger differences could be readily dealt with by transferring resistance from one box to the other in such a way as to keep the sum constant, each ohm transferred being reckoned at 20/18 of a millivolt. It will be understood that the resistance of the bridge-wire was carefully measured in terms of the boxes, that their temperature-coefficients were very nearly the same, and that the wire was tested for uniformity, to insure the above order of accuracy in the determination of differences of E.M.F. of this magnitude.

§ 5. *Thermometry.*

In working to the hundredth of a millivolt, it was necessary to know the temperature of the baths to the hundredth of a degree C. Two thermometers were generally used, one in each water-bath. They were both carefully compared with a platinum thermometer, and their indications were in all cases reduced to the absolute scale.

One of the thermometers was by Geissler, divided to tenths of a degree. This thermometer had evidently been graduated to read temperature on the absolute scale direct. Its errors, after correcting

for rise of zero, were found to be very small and irregular, seldom exceeding 0·01° C.

The second thermometer was by Hicks, divided to twentieths of a degree. Its corrections were found to be very nearly the same as those of the Kew mercurial standard.

Over the range 0° to 30° C. the changes of zero of these thermometers would never exceed 0·01° C., and were, therefore, disregarded. The correction for the length of stem exposed, never exceeding two or three hundredths of a degree, could be applied with sufficient accuracy when required.

The comparisons were made with a platinum thermometer constructed of special wire, which has been repeatedly tested by Professor Callendar, and also by Mr. Griffiths, and by Messrs. Heycock and Neville. The wire is the same as that used in the thermometers made for the Kew Observatory, and its " delta-coefficient " has been taken as 1·50.

The resistance box used was of special design, reading to 0·0001° C. It was exhibited by Professor Callendar at the May Conversazione of the Royal Society in 1893. The box used at Kew for platinum thermometry has recently been constructed on the same model, and has been described in ' Nature,' November 14, 1895. The Kew box differs chiefly in the use of plugs for mercury contacts, and in the absence of the temperature compensating coils.

§ 6. *Comparisons of Board of Trade Cells.*

With this apparatus many more accurate and careful comparisons of Board of Trade cells were made. Several new cells, prepared by H. T. B. and by other students, were compared with those a year old. The newer cells were generally found to have a slightly higher E.M.F. than the old, and in general differences of the same order as before were observed, if the cells were subjected to different treatment. It was noticeable, however, that B.O.T. cells, prepared about the same time in a similar manner, if kept exposed to similar stable conditions, would generally attain the same E.M.F., within one or two tenths of a millivolt, after a day or so in the constant temperature bath at 15° C. The importance of keeping cells of this type at a constant temperature has been shown by Griffiths,* who has obtained very consistent results with B.O.T. cells treated in this manner.

Kahle, on the other hand,† finds differences, amounting to 4 or 5 millivolts in some cases, between the nine B.O.T. cells which he tested *under constant and similar temperature conditions.* Such differences are quite beyond the range of our experience, and we do not

* ' Phil. Trans.,' A, vol. 184 (1893), p. 388.
† ' Wied. Ann.,' vol. 51 (1894), p. 194.

think that they fairly represent the performance of B.O.T. cells *under the conditions that he describes.*

§ 7. *Temperature Coefficients of B.O.T. Cells.*

Fresh determinations of the temperature coefficients of the old cells, in addition to those of the new cells, were made under different conditions and with greater accuracy by means of the constant temperature baths.

For four of the cells in which the end of the zinc rod was at a small distance from the crystals, the mean coefficients obtained on raising the temperature from 15° to 25° C. varied from 0·00046 to 0·00051, and were in practical agreement with previous tests of the same cells under similar conditions. Precisely similar values were obtained after keeping the cells at 25° C. for the night, and then lowering the temperature to 15° C. In another case, after keeping two of the cells at 25° C. for the night, the changes of their E.M.F. from their values at 15° C. were found to be 9·0 and 8·6 millivolts, giving co-efficients 0·00063 and 0·00060 respectively. The greater change is evidently due to the time allowed for diffusion.

On cooling the cells down from 15° to 0° C., allowing them to remain for an hour at the latter temperature, the mean coefficients obtained were invariably much larger. The reason is evidently that the zinc becomes partially imbedded in the crystals at the lower temperature, and is necessarily in contact with a normal saturated solution throughout a considerable portion of its surface. In one case a coefficient as high as 0·00075 was obtained; in another a co-efficient as low as 0·00059. In the latter case the rod was very long, and a considerable length was probably exposed to a supersaturated solution.

That this state of supersaturation is likely to occur, and to persist for a considerable time, is also illustrated by another experiment with a cell containing very few crystals. After keeping the cell in question at 25° C. for a day, it was observed that all the crystals had disappeared, whereas the other cells still showed considerable quantities. On cooling the cell down to 15° C., the E.M.F. rose with a coefficient 0·00040, and then remained steady for some time, no crystals reappearing. After a time a sudden rise in the E.M.F. was observed, and the crystals were seen to have reappeared on the surface of the paste.

On transferring the cells back from the melting ice to the constant temperature bath at 15° C., the E.M.F. of the B.O.T. cells was almost invariably found to be from 2 to 4 millivolts higher than before cooling. The difference was greatest in those cells which contained the greatest quantity of solution, and persisted for several days if

the cell were not disturbed. In one case, after cooling to 0° C., a cell remained nearly 5 millivolts too high when kept for one hour at 15° C. After twenty hours at 15° C., the difference still remained 3 millivolts. In the course of the next two hours it was shaken twice, and returned to within one-tenth of a millivolt of its previous value. On further shaking the cell it was noticed that, if the mercurous sulphate were disturbed so as to come in contact with the zinc, the E.M.F. temporarily fell some 2 or 3 millivolts, but recovered very quickly on the mercurous powder subsiding. This observation illustrates the necessity, now well understood, of keeping the zinc from direct contact with the mercurous paste.

§ 8. *Illustration of Diffusion-lag.*

To illustrate the extreme slowness of the diffusion process, and to show that all saturated cells have really the same coefficient, if sufficient time be allowed for diffusion, the following experiment was tried. Two exactly similar Board of Trade cells of normal E.M.F. were taken from the constant-temperature bath at 14° C., and placed in the other bath at 25° C. Two or three observations were taken each day of their subsequent changes of E.M.F. After the lapse of two days, one of the cells was occasionally shaken, and rapidly gained the correct value of the E.M.F. of a saturated cell at 25° C. The other cell was left undisturbed, the temperature being maintained constant to one-fiftieth of a degree Centigrade. The E.M.F. of the latter cell fell slowly and almost uniformly as the diffusion proceeded, but it was not until after the lapse of nearly a fortnight that it reached the correct value.

The annexed curves (Fig. 1) illustrate the rate of diffusion in these cells. The abscissæ represent time in days; the ordinates, fall of E.M.F. in millivolts from 15° C.

For the sake of comparison, a cell of a different type, which we designate "B.O.T. crystal," was submitted to the same treatment at the same time. The straight line BC relates to this "crystal" cell. It will be seen that it shows no appreciable diffusion-lag. In fact its E.M.F. had arrived in twenty minutes within a tenth of a millivolt of its final value.

Curve No. (1) relates to the B.O.T. cell which was shaken. The points at which the shaking took place are marked by the sudden falls of E.M.F.

Curve No. (2) relates to the cell which was left undisturbed. The rate of diffusion would probably have been much slower and more uniform, but for the slight vibration due to the running of the stirrer in the water-bath. The changes in the rate of diffusion shown at the points 2·5 days and 8·5 days were probably due to excessive stirring about those dates.
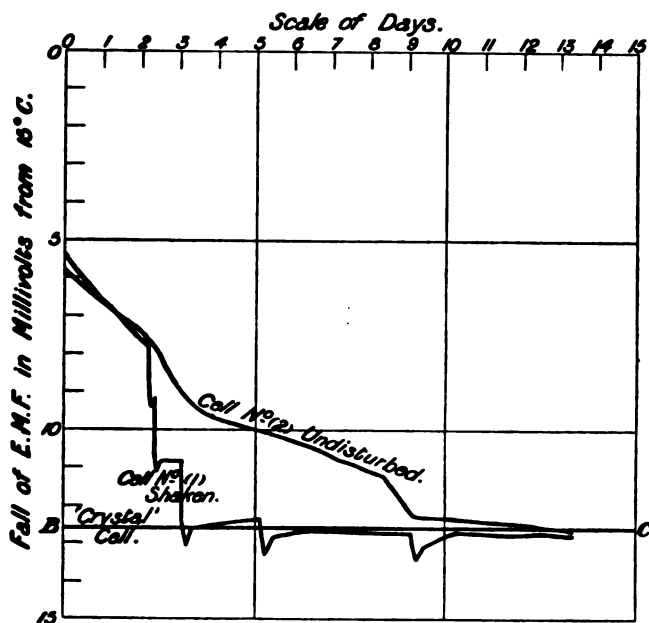
FIG. 1.—Curves showing Diffusion-Lag of two B.O.T. Clark Cells.

We have also tested several of the Muirhead portable cells in a special water-jacketed air bath, the temperature of which was regulated by a thermostat. We find that for comparatively rapid changes of temperature, such as 10° C. in two hours, they have a temperature-coefficient of 0·00050, on the average, between 10° and 30° C. They have also a slow diffusion-lag similar to other B.O.T. cells. It must be remembered, however, that these cells, owing to their form, are not intended for the most accurate laboratory tests, and that they are quite sufficiently constant for the purposes for which they are generally used.

It is sufficiently evident from the examples above given that a Clark cell of the B.O.T. form containing clear solution cannot be said to have a definite temperature coefficient. The change of E.M.F. is seen to depend on the previous history of the cell, on the rate of change of temperature, and on the quantity of solution and relative size and position of the zinc rod. The temperature-coefficient at 15° C. may have any value, from 0·00040 to the value 0·00078, which is generally taken. If the latter value of the coefficient is regularly used, errors which are relatively considerable in accurate work may readily be made, even if the rate of change of temperature is only 4° or 5° a day. There is no doubt that, provided suffi-

-cient time is allowed, and sufficient crystals are present, the higher
value is the more correct one to use; but we are inclined to think
that many observers are not aware how extremely slow the process
-of diffusion really is, and how considerable a time is required for the
attainment of the limiting value.

§ 9. *Board of Trade "Crystal" Cells.*

The defects of the ordinary B.O.T. form of Clark cell in this
respect of diffusion-lag have long been recognised, and various
methods have been proposed to remedy the disadvantages resulting
from it.   Lord Rayleigh himself preferred the H-form of cell, in
which the zinc is buried under a layer of crystals.   This form of
-cell has been adopted by the Berlin Reichsanstalt, and has been
shown by Kahle to be practically free from the defects of the other
pattern.   We have made several cells of the H-form, but we are
strongly of opinion that, besides being more difficult to make, they
are not so convenient to work with as the B.O.T. test-tube pattern.

On considering the matter in the light of the preceding obser-
vations with regard to the great differences in the temperature-
-coefficient produced by different degrees of immersion of the zinc
rod in the crystals, it occurred to us that the diffusion-lag of the
B.O.T. pattern might be entirely removed by a very simple modi-
fication of procedure ; so simple, in fact, that it would seem scarcely
to deserve mention, were it not that of the many hundreds of
B.O.T. cells which we have seen and examined, not one has been
constructed in the manner to be described.

The modification we have adopted in these cells, which we term
Board of Trade "crystal" cells, consists simply in filling the cell
above the mercurous paste with moist crystals, instead of with satu-
rated solution.   Under these circumstances no part of the solution
can remain either supersaturated or unsaturated for any appreciable
time.

We have subjected these and other cells to the severest tests, and
the most sudden variations of temperature, such as 0° C. to 25° or
30° C., and we find that the B.O.T. cells, when filled in this manner
with crystals, have no appreciable diffusion-lag, and are not sur-
passed in quickness by any other form.

§ 10. *Preparation of Crystal Cells.*

The procedure which we adopt in making these crystal cells
differs only in one or two small details from that prescribed in the
Board of Trade memorandum on the Clark cell.

A stock solution with mercurous sulphate paste is prepared

exactly as therein described. When cool, the supernatant liquid may be decanted off to be used for the preparation of crystals. A supply of suitable crystals is easily obtained at any time by taking this solution, or any other solution of zinc sulphate which has been neutralised and treated with mercurous sulphate at 30° C., and cooling it down to 0° C. The liquid is then decanted off, and the crystals drained on a piece of filter-paper. This method leaves them sufficiently moist for the purpose. The crystals are filled into the cell through a glass tube or funnel to a depth of about 2 cm., and a hollow is made in the surface with a glass rod to facilitate the introduction of the zinc. When the cell has settled it should be free from air-bubbles, and should show the merest film of liquid above the surface of the crystals. Inasmuch as zinc sulphate does not tend to form any hydrate higher than the hepta-hydrate between the limits 0° and 30° C., the crystals will remain equally moist, or very nearly so, between these limits.

After soldering the platinum wire on to the zinc rod, we prefer to seal the end of the zinc rod with marine glue into a glass tube which nearly fits it. The object of this is to make the best possible seal to protect the solder joint, which may otherwise be injured by the creeping of the solution. The upper part of the glass tube is then sealed on to the platinum wire to form a mercury cup. The glass tube also forms a convenient handle to use when inserting the cork and the zinc rod into the cell. We prefer to amalgamate the zinc rod, as this proceeding appears to protect the zinc from local action, and to give more uniform results. We may here remark that in the B.O.T. cells containing solution the zinc rod, even if amalgamated, rapidly becomes corroded near the top by local action. In the cells filled with crystals, on the other hand, the zinc remains perfectly bright and clean.

§ 11. *Decomposition of Mercurous Sulphate.*

After neutralising the zinc sulphate solution with zinc oxide, we prefer to filter in a jacketed funnel at 40° C., for the sake of obtaining a stronger solution. Mercurous sulphate is then added to remove any traces of zinc oxide or other impurities which have any action upon it. We have observed that if, after the addition of the mercurous sulphate, the solution be heated to between 35° and 40° C., a slight change may be noticed in the appearance of the mercurous sulphate. The filtrate, when cooled to 15° C., may remain clear, but, if further cooled to 0° C., a yellow turbidity makes its appearance, showing that the mercurous sulphate has, in all probability, been partly decomposed by exposure to the higher temperature. If, on the other hand, the zinc solution has not been heated above 30° C.

with the mercurous sulphate, the filtrate will remain clear when cooled to 0° C. It appears probable that if a Clark cell is heated above 30° C., or is made from paste which has been so heated, its E.M.F. may be affected by a similar cause. The temperature of 30° C. appears, however, to be a perfectly safe limit. It is not unlikely that the decomposition is determined by the presence of zinc oxide, as we have rarely observed changes of more than one or two tenths of a millivolt even after heating a cell to 50° C.

§ 12. *Tests of B.O.T. Crystal Cells.*

Several of these crystal cells were made by H. T. B., and later by the advanced electrical students in the ordinary course of their work. The cells so set up at different times by different students were rarely found to differ under any conditions by so much as the tenth of a millivolt from the mean at any given temperature. If, as occasionally happened, a new cell, within an hour or so of sealing up, was found to have an E.M.F. as much as two tenths of a millivolt too high, it was short-circuited for half an hour or so with a piece of copper wire. This procedure invariably had the effect of reducing the E.M.F. to its normal value.

It might naturally be supposed that with so small a quantity of solution, these cells would be seriously affected by short-circuiting. We have found, on the contrary, that they are much less affected than the ordinary B.O.T. form or than unsaturated cells. The crystal cells on short circuit were found to give a current of about 5 or 6 milliampères, falling gradually to 2 or 1 in the course of an hour. On removing the short circuit the cells instantly recovered to within a millivolt of their normal value, so quickly, in fact, that it was found impossible by the balance method to obtain any intermediate readings showing the rate of recovery. In less than five minutes the value had generally recovered to within a tenth of a millivolt of the normal.

The ordinary B.O.T. cells of the same size, containing clear solution, were found to give a similar current on short circuit, but the recovery was never so rapid or perfect.

§ 13. *Temperature Coefficient of Crystal Cells.*

Having satisfied ourselves by various preliminary trials that the crystal cells were practically free from diffusion-lag, and finding that the temperature coefficient between 15° and 0° C. appeared to be somewhat higher than that given by Kahle and other authorities, we determined to make a systematic series of observations under definite and uniform conditions.

For this test four crystal cells of different dates were selected, differing as widely as possible (one-tenth of a millivolt either way) from the mean at 15° C. Six other cells were set apart as standards of comparison, and were kept at a constant temperature of 15° C. night and day throughout the series of observations. The cells were all of the same form and dimensions as the B.O.T. cells above described, and were all sealed with marine glue.

At starting the mean of the test cells at 15° C. agreed to one hundredth of a millivolt with the mean of the standard cells. The four test cells were then immersed in melting snow, and comparisons of their E.M.F. with the standards were made at intervals of six, eighteen, and twenty-four hours after immersion. For the next twenty-four hours they were kept at a temperature of 5° C., next day at 10° C., then for three days at 15° C., then for a day each at 20°, 25°, and 30° C. In this manner the comparisons at each point were made as nearly as possible under similar conditions.

The small differences which the cells possessed at starting were maintained, within two or three hundredths of a millivolt, throughout the series of observations. They were possibly due to inequalities of age, or to slight differences in the preparation of the solutions and crystals, or to the fact that the platinum wires in the mercury cups were not amalgamated. No systematic difference in their temperature-coefficients could be detected.

At each point the mean of the observations taken at the end of the first six hours showed a slight lag, as compared with the observations taken at eighteen and twenty-four hours, amounting on the average to nearly two hundredths of a millivolt. It is necessary to remark, however, that a difference of one hundredth of a degree of temperature means rather more than one hundredth of a millivolt; and that this apparent lag, corresponding to less than 0·02° C., may have been due to some constant error of observation of temperature under different conditions in the morning and evening. In any case it is evident that the diffusion-lag, if any, is so small that it would be quite useless to consider it in any case, unless the greatest precautions were taken to secure a constant and uniform temperature, and to measure the temperature to at least 0·01 C.

After the three days at 0°, 5°, and 10° C., the mean of the four cells returned in less than twenty-four hours to within one hundredth of a millivolt of the mean of the standard cells. After three days at 15° C. the mean values were identical. At the conclusion of the observations at 30° C., the cells were replaced in the bath at 15° C. In fifteen hours the mean value had returned to within two hundredths of a millivolt of the standards.

Some two months after the above series of experiments, a second set of observations at 30° C. was taken with three of the same cells, by

way of verification. The cells at 15° C. were still found to preserve their relative differences to within one or two hundredths of a millivolt, and the mean still agreed with that of the standards. The difference at 30° C. was observed directly in terms of the resistance boxes, as well as in terms of the bridge-wire, at each observation. The fall of E.M.F. found was less by four hundredths of a millivolt in 19·4 millivolts, than on the preceding occasion. After twenty-seven hours at 30° C. the cells, when replaced in the 15° C. bath, returned in twenty hours to within two hundredths of a millivolt of their previous values.

§ 14. *Quickness of Recovery.*

It must not be hastily assumed from the foregoing observations that the cells in each case took nearly a day to recover their original values at 15° C. The observations were taken in this particular series after the lapse of several hours in order to make sure that the cells had not undergone any permanent change as the result of prolonged exposure to 0° and 30° C.

We have made several special tests with regard to this point. We find that B.O.T. crystal cells of this size, set up in test-tubes nearly 2 cm. in diameter, when suddenly transferred from melting snow, or from the other bath at 30°, or even 40° C., back to the constant temperature bath at 15° C., return to within a tenth of a millivolt of their previous values *in less than ten minutes.*

For cells of a smaller size, of which we have made several, the recovery is still more rapid. It appears to be chiefly a question of the time required for the change of temperature. After exposure to a temperature above 15° C., the cells frequently return, at 15° C., in less than half an hour to within one or two hundredths of a millivolt of their previous values. After exposure to a temperature below 15° C., the recovery is a little slower. This might naturally be expected, as crystallisation is generally more rapid than solution.

We have also taken a series of observations at 40° C., though this temperature lies outside the limits of practical utility. On suddenly raising the temperature to 40·6° C. the value observed after ten minutes was one millivolt higher than the final value. The next observation was taken after three hours, by which time it was found, on subsequent reduction, that the E.M.F. had become constant. The E.M.F. at 40·60° C. was found to be 35·81 millivolts lower than at 15° C.

The recovery on returning to the 15° bath after a day at 40·6 C. was quite unexpectedly rapid. In ten minutes the value was found to be within a tenth, in two and a half hours within a hundredth of a millivolt. Later observations showed no further change.

§ 15. *Results of Observations.*

Table I.—Temperature-variation of B.O.T. Crystal Cells.

Tempera-ture C. corrected to nitrogen-scale.	Difference in millivolts from value at 15° C.		Difference from lineality.		Difference from formula (*p*).
	Observed (O).	Formula (L).	Observed (O)—(L).	Calculated formula (*p*).	
0·00°	+16·62	+18·00	−1·38	−1·40	+0·02
5·17	+11·15	+11·80	−0·65	−0·60	−0·05
9·89	+ 5·92	+ 6·13	−0·21	−0·16	−0·05
20·43	− 6·70	− 6·52	−0·18	−0·18	−0·00
24·75	−12·25	−11·70	−0·55	−0·59	+0·04
29·90	−19·42	−17·88	−1·54	−1·38	−0·16
29·86	−19·32	−17·83	−1·49	−1·37	−0·12
40·60	−35·81	−30·72	−5·09	−4·07	−1·02

Fig. 2.
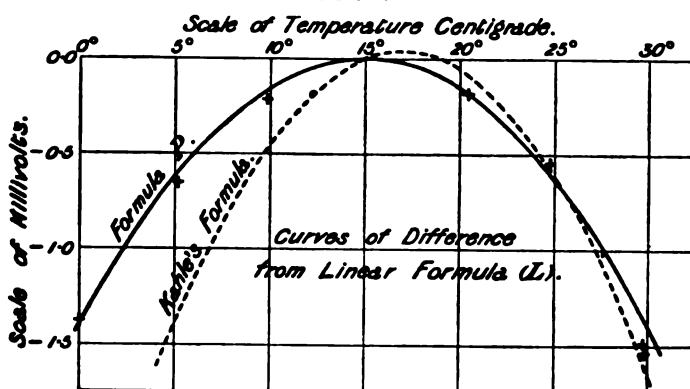
The results of these experiments on the temperature-variation of the E.M.F. of B.O.T. crystal cells are given in the above table, and are plotted in the accompanying curve (fig. 2). The curve is drawn to show not the whole temperature-variation of the E.M.F., but the defect of the change from lineality.

The observed E.M.F. in volts at a temperature $t°$ C. is less at all points than that calculated by the simple linear formula,

$$E_t = E_{15} - 0.001,200\ (t-15). \quad \ldots\ldots\ldots\ldots \text{(L)}.$$

The difference from this linear formula is approximately represented by the addition of a parabolic term,

$$-0.0000062\ (t-15)^2 \ldots\ldots\ldots\ldots\ldots \text{(P)}.$$

The full curve in fig. 2 represents this difference-term on a scale of $2\frac{1}{5}$ cm. to 1 millivolt. The crosses represent the results of actual observation at the different points. The mean difference of the observations from the curve, if we except those at 30° C., is only three-hundredths of a millivolt. The observations at 30° C. differ by more than one-tenth of a millivolt, and those at 40° C. by a whole millivolt from the simple parabolic curve. These differences cannot, we think, be explained as being due to errors of observation. This is proved by the accuracy with which the cells returned to their original values at 15° C., and also by the agreement of the twenty or thirty different readings at each point. Moreover, the observations have been repeatedly verified by others, not shown on the curve, with cells of different types, to within one or two-hundredths of a millivolt. We conclude that no simple parabolic formula can be made to fit the observations throughout the range 0° to 40° C. Over the range 0° to 28° C., however, the differences, even if real, are not of great importance, and we may take the formula,

$$E_t = E_{15} - 0\cdot001200\,(t-15) - 0\cdot0000062\,(t-15)^2, \ldots \ldots \text{(L)} + \text{(P)}$$

as representing the temperature-variation of the E.M.F. of these cells within about one-twentieth of a millivolt between these limits. The symmetry of the curve shows that we may take the very convenient round number $1\cdot200$ millivolt, for the change of E.M.F. per 1° C. at 15° C.

Taking formula (P), we find for *the temperature-coefficient at $t°$ C.,*

$$d/dt\,(E_t/E_{15}) = -0\cdot000837 - 0\cdot0000087\,(t-15),$$

and for *the mean temperature-coefficient between $t°$ and 15° C.,*

$$(E_t/E_{15}-1)/(t-15) = -0\cdot000837 - 0\cdot0000043\,(t-15).$$

It is generally, however, more convenient to use the formula $(L+P)$: Between the limits of 12° and 18° C. we may use the simple linear formula (L), without risk of making an error greater than one-twentieth of a millivolt. If, however, we were to use the temperature-coefficient $0\cdot00076$ (which is very commonly taken) over the same range, the error might amount to nearly three-quarters of a millivolt.

§ 16. *Results of Previous Observers.*

The formula given above for the temperature-coefficient differs from that of previous observers chiefly in the direction of making the change of E.M.F. more nearly uniform.

Lord Rayleigh[*] tested two cells under similar conditions of slow temperature change. For one cell he found the temperature-coefficient at t given by the formula $0\cdot00083 + 0\cdot000018(t-15)$, which

[*] 'Phil. Trans.,' vol. 176 (1835), p. 794.

agrees with our value, except that the rate of change of the coefficient is twice as great. For the other cell he found a coefficient 12 per cent. smaller with a similar rate of change. His cells, in which the zinc was pushed down into the paste, would be certainly less liable to diffusion-lag than the ordinary B.O.T. pattern, and might possibly possess a different coefficient; but we think that the difference is most probably to be explained by diffusion-lag, and in that case the higher value would be the more correct.

Glazebrook and Skinner find the value 0·00076 for the mean coefficient between 0° and 15° C., under conditions in which diffusion-lag would be approximately eliminated. Our value between these limits is 0·000773.

Our value for the coefficient at 15° C., namely, 0·000837, is also in fairly close agreement with the value 0·00082 for an H-cell at 15° C., found by Fleming. The lower values obtained by many observers for ordinary B.O.T. cells, are doubtless vitiated by the effects of diffusion-lag, and are, in this respect, in agreement with our own results for such cells under similar conditions as given in a previous section.

The observations of Kahle[*] are probably the most systematic. He finds for the E.M.F. in volts at $t°$ C. the formula

$$E_t = E_{15} - 0\text{·}00116\,(t-15) - 0\text{·}00001\,(t-15)^2 \ \ \ldots\ldots\ \ (\text{K}).$$

The difference between this formula of Kahle and the linear formula (L) is shown by the dotted curve in fig. 2. It will be seen that the agreement with our observations is very close between 25° and 30° C., but that the value of the coefficient at 15° C. is somewhat smaller. Below 10° C. the divergence is very marked. The formula of Kahle gives a change of only 15·15 millivolts between 0° and 15° C., corresponding to a mean coefficient of 0·000704, values which are evidently much smaller than those given above.

We do not think, however, that it is necessary to assume that there is any real difference of behaviour between our cells and those tested by Kahle. The discrepancy is more probably to be explained by the fact that the observations on which the formula of Kahle is founded were taken between the limits 12° and 28° C., under conditions less favourable to the cells. Between these limits, so far as we are able to judge, we are in agreement with Kahle, within the limits of accuracy of his observations. Kahle does not give any detailed observations or any statement of the probable error of his results, but it is possible to form a general idea of the limits of accuracy from the account which he gives of his method. The cells were kept immersed in paraffin baths, regulated by means of " Rohrbeck " thermostats. The temperature seldom varied more than one degree from day to

[*] 'Wied. Ann.,' vol. 51 (1894), p. 197.

day in either bath, or by more than a tenth of a degree in the course of an observation. From other observations given by Kahle[*] it would appear as though his cells were subject to a temperature or diffusion-lag of the order of half a millivolt, when the temperature was changing at the rate of 1° C. per hour. From these and other considerations it is evident that Kahle did not aim at an order of accuracy higher than one or two-tenths of a millivolt, and that his formula could not be expected to give correct results beyond the limits of observation.

Thus, although his formula is practically correct between the limits of his observations, namely 12° and 28° C., it is quite possible that it may be as much as 10 per cent. in error at 0° C. On the other hand, we regard it as quite impossible that our observations at this point should be in error by even a tenth part of the amount, namely 1½ millivolts, by which they differ from the formula of Kahle. Again, although the observations of Glazebrook and Skinner between 15° and 0° C. may have been affected to a slight extent by diffusion-lag, it is plain that the effect of diffusion-lag, if any, must have been to reduce the extent of the change, and could not explain the fact that the change which they observed was so much larger than that given by Kahle's formula, and so nearly in agreement with our own.

In this connection it is necessary to refer to an opinion which we have often encountered in conversation and otherwise, and which is possibly still current, namely that the observations of Glazebrook and Skinner are in precise agreement with those of Kahle on this point. For instance, Schuster[†] quotes correctly Kahle's formula for the mean temperature-coefficient between $t°$ and 15° C., namely,

$$a = 0.000814 + 0.000007 \ (t-15),$$

and states that Glazebrook and Skinner's coefficient $(a = 0.00076)$ refers to a mean temperature of 7.5° C., and is identical with the above at that temperature. This is obviously true if we put $t = 7.5°$ in the formula, but not if we put $t = 0°$. The mistake appears to have arisen from the above formula having been inadvertently described by Kahle as being *the temperature-coefficient at t*, instead of *the mean coefficient between t and* 15° C. But although the words, "*Für eine beliebige Temperatur t*," used by Kahle, may, perhaps, be ambiguous, the complete formula (K), from which the other is derived, leaves no possible doubt as to the true meaning.[‡]

[*] *Loc. cit.*, p. 199.

[†] 'Phil. Trans.,' A, vol. 186 (1895), p. 458.

[‡] Note added Sept. 20, 1897. In a more recent number of 'Wied. Ann.,' Oct., 1896, Kahle states incidentally that he has found by direct comparison the difference 16.6 millivolts between 0° and 15°, instead of 15.1 as given by his previous formula. No details of observations are given.

§ 17. *Hermetically Sealed Cells.*

We have always felt somewhat dissatisfied with the usual practice of sealing up standard cells with marine glue or paraffin wax. Provided that the marine glue sealing is carefully and conscientiously performed, it may, doubtless, remain good for a considerable time, if the cell is not exposed to extreme variations of temperature. We have not, however, ourselves succeeded in making the marine glue seal stand many repetitions of the 0° to 30° C. treatment. Cells which have been thus treated for a month or two have invariably shown some signs of creeping. Except in extreme cases, this creeping does not appear to produce much effect on the E.M.F. of saturated cells, but in the case of unsaturated cells the effect is serious. The set of twelve Carhart-Clark cells in our collection, though evidently prepared and sealed with the greatest skill and care, have all suffered from the creeping out of the solution in the lapse of two years. One of the cells is now 5 millivolts below its normal value. It is only fair to add that, owing to the extremes of the Montreal climate, they have been subjected to an annual temperature range of 5° to 27° C., and that all the Muirhead cells in our collection are similarly affected.

We have succeeded in making several forms of hermetically sealed cells, and we are of opinion that such cells are much to be preferred as standards to those sealed in any other way. The following are the principal varieties on which we have made experiments.

(1) *Cells of the* H-*Type with Zinc Amalgam.*—We prefer to make this cell in the form (χ) of an inverted Y. Fine platinum wires are first sealed into the lower extremities of the inverted Y, the limbs are then filled with zinc amalgam and crystals of zinc sulphate, and with mercury and mercurous paste as usual. When sufficient materials have been introduced, the middle leg is sealed off. This inverted Y-form is much easier to make than the H-form, as it involves only one T-join. In making these cells, we prefer to use lead glass tubing about 5 to 8 mm. in bore. Hermetically sealed cells of this form were made many years ago by Wright[*] for the purpose of testing the effect of dissolved air on the E.M.F.

W-*Form.*—When intended for immersion in a water bath, the lower limbs of the inverted Y are continued upwards beyond the seal to a height of 4 or 5 inches forming a W. The upturned limbs are partly filled with mercury, and are used for making connections.

As the result of our experience with cells of this description, we are not inclined to recommend the use of cells containing zinc amalgam at temperatures above 25° C. or below 10° C. As Lord Rayleigh has observed, these cells show a very remarkable tendency

[*] ' Phil. Mag.,' vol. 16 (1883), p. 28.

to crack in the platinum seal of the leg containing the zinc amalgam, especially if exposed to low temperatures. We do not think that they could be trusted to stand the Montreal climate. Very few of our cells constructed on this pattern have survived a month or two of the 0° to 30° treatment, if they contained more than a mere button of amalgam.

(2) *H-Form Cells with Zinc Rod.*—We have generally found cells made with zinc rod to be more reliable. The zinc rod is cast in a small glass tube of suitable size. The platinum wire, previously enclosed in a capillary glass tube, is thrust into the fused zinc. When cool, the glass mould is broken off, and the zinc rod cleaned and amalgamated, and introduced into one leg of the H. The other leg is partly filled with mercury to which connection is made by a platinum wire in a glass tube after the Board of Trade method. The other materials are filled in as usual. The two legs of the H are then fused up at the top, the upper portions serving as mercury cups.

We prefer to use tubes of very small dimensions, and to make the horizontal connecting limb as short as possible. This cell is not very easy to make, owing to the double fusion on to the platinum wires after the materials have been filled in, when the cell cannot be inverted. If, however, the tubes are made sufficiently small, it is a very convenient and sensitive form of cell.

(3) *Board of Trade Form with Crystals.*—The single tube form with the zinc rod cast in a similar way on to a glass capillary, is equally efficient if filled with crystals, and is much easier to seal. There is only one tube to seal, and it is possible to get at it evenly from all sides. There is generally no difficulty in keeping the two wires separate, provided that the capillaries through which they pass have been drawn sufficiently thick and strong. Both the capillaries may be expanded into mercury cups at the top, or the outer tube itself may conveniently be used to form the mercury cup for the zinc terminal.

Portable Form.—This cell is still more easily made in a portable form, in which the mercury is replaced by amalgamated platinum. The cell may then be made upside down, the difficult seal being made first, before the materials are filled in. The process of making the cell is briefly as follows. A platinum wire is sealed into a thick glass capillary with a small mercury cup at one end. The wire is left projecting some 2 or 3 cm. beyond the glass at each end. One end is then hammered flat to serve as the positive element, and both ends are amalgamated by heating and plunging in mercury. A platinum wire capillary without a mercury cup is cast into a small zinc rod, and the free end is amalgamated. A glass tube of suitable size and length to form the cell is melted down in the middle till it

is of sufficient size to just admit the passage of the two capillaries from opposite sides. The capillaries are then held in their proper relative positions while the seal is completed, and the free ends of the wires are coiled down in their respective mercury cups. The lower end of the tube is then drawn out slightly to facilitate the final sealing off.

The materials are filled in after the usual method, but in the reverse of the usual order. Moist crystals of zinc sulphate are packed round the zinc till it is covered to a depth of about a centimetre. After inserting about half the mercurous paste, the flattened and amalgamated platinum wire is coiled down into a suitable position, and more paste is added. The end of the cell close to the point where it is to be finally sealed, is preferably filled with moist crystals instead of paste. The object of this is to avoid leaving any of the paste close to the seal, where it might suffer decomposition from the heat in sealing off. Any excess of solution is dried off with filter paper, and the narrow neck is then sealed off with a fine flame.

On the whole, we prefer this portable form of cell to any of the other forms we have tried. The shape of the cell and its small size make it very convenient. The seal is comparatively easy to make, and the narrow glass neck separating the cell from its connexions is also an advantage, as it diminishes the risk of any error of temperature arising from conduction along the tube.

We have not found that there is any advantage in using mercury as compared with amalgamated platinum. The cell has a higher internal resistance, and gives a smaller current on short circuit, but the recovery appears to be equally rapid and complete. We cannot find any systematic difference in the electromotive force at any temperature. If anything, the amalgamated platinum has the advantage over the mercury, as the purity of the mercury is then comparatively unimportant, and redistillation is unnecessary. There does not appear to be any advantage gained, in our experience, by using a strip of platinum foil in place of the fine flattened wire.

§ 18. *Tests of Hermetically Sealed Cells.*

We have similarly tested several saturated cells of the H-form, and various other patterns above described as hermetically sealed cells. We find that they all show the same temperature change of E.M.F. as the B.O.T. crystal cells. The agreement in nearly every case is within one or two-hundredths of a millivolt even at 0° and 30° C., the limits of the range.

The largest divergence was found in the case of a portable cell of type (3), with an amalgamated platinum wire in place of mercury. In this cell, both at 0° and 30° C., the difference from 15° C.

exceeded by one-tenth of a millivolt that of all the other crystal cells we have tested. Even in this case the cell was at least consistent with itself. It was first tested at 15°, 30°, 15°, 0°, and 15° C., allowing a day at each temperature. It was a new cell, the first of this type which we tested, and its value at the time was nearly three-tenths of a millivolt higher than the standard. The value found at 0° C. was 16·72 millivolts above, and at 30° C. 19·72 millivolts below its value at 15° C., instead of +16·62, and −19·58 millivolts respectively. The cell being of a very small and sensitive form, the test was repeated a few days later in the reverse order, allowing only half an hour at each temperature. The values found in the second test were +16·71 and −19·70 millivolts respectively. We thought at first that the discrepancy might be due to some inherent peculiarity of this type of cell. We have since tested other cells of the same type and size, with results which agree to 0·02 millivolt with the B.O.T. crystal cells. The agreement is not confined to points 0° and 30° C. For instance, the difference found at 24° C. was 0·05 of a millivolt less than that calculated by the formula (P). The B.O.T. crystal cells at this point show a difference of 0·04 millivolt from the curve (P) in the same direction. We consider that the divergence of one-tenth of a millivolt in the case of this particular cell must be regarded as exceptional.

§ 19. *Importance of Constant Conditions of Temperature.*

We have quoted the above test partly as an illustration of the kind of agreement between cells of different types which it is possible to attain with suitable cells under definitely known conditions of temperature.

In attaining this order of accuracy the chief difficulty lies in the certain determination of the temperature of the cells. To attain a certainty of the order of 0·01° C., the following conditions are necessary :—

1. The cells should be of an elongated form, and should be deeply immersed in a bath of liquid, which is constantly and vigorously stirred.

2. The temperature must be kept constant to 0·01° C., and the thermometer used must be read and corrected to the same order.

If the cells are not deeply immersed their temperature will be affected by external conditions. If the liquid is not constantly stirred it will tend to be hotter at the upper surface, especially if the liquid be very expansible, like paraffin, and the bath be hotter or colder than its surroundings. If the temperature is changing the cells will not be of an uniform temperature throughout, and will lag behind the thermometer, unless they happen to be of a smaller size.

The electrical conditions are not less important, but are more easily realised and maintained. Our galvanometer is sensitive to much less than a millimetre of the bridge-wire (one hundredth of a millivolt). Great attention is paid to the perfection of the insulation, and to the avoidance of thermo-electric effects, which may readily amount to more than ten microvolts. We may here remark that in testing any new batch of cells it is quite impossible to tell, till the results are worked out, whether they are in agreement with others. The many coincidences found cannot therefore be the result of bias on the part of the observer.

We think we may fairly claim for the Clark cell an order of consistency approaching one-hundredth of a millivolt in the temperature changes of its E.M.F.

§ 20. *Clark Cells in which the Solution is of Constant Strength.*

It is well known that Clark cells, not containing crystals, in which the solution does not change its strength with change of temperature, have the advantage of possessing a temperature-coefficient which is less than half that of the saturated cells. They are also practically free from the effects of diffusion-lag, as the density of the solution is always nearly uniform.

The best known cell of this type is the Carhart-Clark cell, in which the zinc sulphate solution is chosen as being saturated at $0°$ C. An error of $2°$ C. in the temperature at which the solution is saturated, will make an error of only one millivolt, approximately, in the E.M.F. of the cell.

We have prepared several cells of this type at different dates and in different forms, with separately prepared solutions. In cells so prepared, of similar patterns, we have not as a rule found differences greater than two or three-tenths of a millivolt. These differences were probably due as much to other cause as to difference of strength of solution. We have generally sealed the cells hermetically to avoid creeping of the solution, which has a tendency to lower the E.M.F. in the case of unsaturated cells.

It is evident that these cells must, on the whole, be less accurately reproducible than the saturated cells. We have also found that they are more liable to undergo slight changes of E.M.F. as a result of short-circuiting, or of exposure to high or low temperatures. They appear, in fact, to be less stable than the cells containing crystals.

We have also prepared experimental cells with solutions weaker than the cell saturated at $0°$ C. We have observed in these cells a similar instability, becoming more marked as the solution is weakened. We are inclined to attribute this instability to a difference in solubility or diffusivity of the mercurous sulphate in the weaker solutions.

We also found it possible to obtain observations of the change of E.M.F. of cells saturated at 15° and 30° C., under conditions in which the solution was considerably supersaturated. The cell saturated at 15° C. was kept for several hours at 0° C. without showing any trace of crystallisation. This cell agreed at 15° C. with the saturated cells, and gave very consistent readings throughout the range 0° to 30° C. The change of E.M.F. per 1° C. was found to be 0·567 of a millivolt between 0° and 15° C., and 0·560 of a millivolt between 15° and 30°. As the whole change of E.M.F. between 0° and 30° C. was only 16·90 millivolts, and as the E.M.F. of the cell rose by one-tenth of a millivolt after keeping for six hours at 0° C., the observations may be taken as showing that the temperature-coefficient of this cell, whether in the supersaturated or unsaturated condition, is practically constant over the range 0° to 30° C. This is in marked contrast with the case of the cells containing crystals.

The tests of the cells saturated at 0° C. were very fairly consistent, but not quite so good as those of the cells saturated at 15° C. They showed a mean temperature change of E.M.F. per 1° C. of 0·543 of a millivolt. There was no decided evidence of any variation of the temperature-coefficient over the range 0° to 30° C. Carhart gives the formula :—

$$E_t = E_{15} \left[1 - 1·000387 \, (t-15) + 0·0000005 \, (t-15)^2 \right],$$

which would make the temperature-coefficient diminish slightly as the temperature rises. (Change of E.M.F. 0·56 mv. per 1° C.).

The tests on the weaker cells were much less consistent, owing to the instability of E.M.F. above referred to. The results of the tests pointed to a mean change of 0·55 of a millivolt per 1° C. The change observed between 0° and 15° C. was sometimes greater and sometimes less than that between 15° and 30° C., but there was no decided tendency either way. After keeping for some time at 0° or 30° C. these weak cells sometimes showed permanent changes amounting to as much as half a millivolt.

§ 21. On the Density of Solutions of Zinc Sulphate.

A knowledge of the density of solutions of zinc sulphate is required in order to trace the relation between the changes of E.M.F., which depend on change of strength and density of the solution. This point has been investigated by two of Professor Carhart's students, the result of whose work has been published by Professor Carhart.[*] These observers find for a cell saturated at 15° C. an E.M.F. nearly five millivolts higher than that of a cell containing crystals, and a density which appears to be correspondingly low. For this and

[*] 'Proc. Amer. Elect. Eng.,' 1892, p. 615.

other reasons, we thought it would be desirable to repeat the deter-
minations. The observations of Lannoy,* though evidently under-
taken with great care, did not extend to the case of solutions as
dense as those occurring in Clark cells. Other observers appear to
have confined themselves chiefly to the case of very dilute solu-
tions.

According to the views of Valson and Bender, which are quoted
by Ostwald and other authorities, the density of a salt solution, such
as zinc sulphate, may be additively deduced from the observed den-
sities in the case of some standard solution (*e.g.*, a solution of
ammonium chloride), by means of two moduli representing the acid
and the base respectively. We have calculated the densities accord-
ing to the values which they give for the moduli at 18° C., but it
appears that the results are only a rough approximation, and miss
what seems to be the most characteristic feature of the change of
density.

In determining the relation between density and strength of
solution, the chief difficulties to be encountered are in the exact
measurement of the strength. If the composition of the solution is
determined by weighing out known quantities of the hydrated salt
into a litre flask, it is very possible that errors may arise from
evaporation or efflorescence, or from the presence of other hydrates.
In order to avoid these possible errors, we adopted the much more
laborious method of evaporating a known weight of solution to
dryness at 100° C., assuming that the residue was the monohydrate.
Two determinations were made in this manner for each solution
tested, and in addition, two control experiments were made in
which the strength of the solution was measured by estimating the
sulphate by means of barium chloride. The following table contains
the results of these determinations for seven different solutions.

Table II.—Density of Zinc Sulphate Solutions.

$ZnSO_4$ per cent. of solution. (p.)	Density of solution (d).	$ZnSO_4$ gram per c.c. $pd/100$.	Difference 0.9982 $+ w - d$.	Calculated by (D) formula.
6·35	1·0653	0·0677	0·0006	0·0000
8·46	1·0896	0·0923	0·0009	0·0000
13·49	1·1522	0·1557	0·0017	0·0006
17·69	1·2020	0·2130	0·0092	0·0070
23·75	1·2872	0·3062	0·0172	0·0174
27·27	1·3418	0·3667	0·0231	0·0242
33·21	1·4400	0·4790	0·0372	0·0367

* Ostwald, 'Zeit. Phys. Chem.,' Nov., 1895.

The first column contains the percentage by weight, p, of $ZnSO_4$ in grams per 100 grams of solution, as deduced from the observations on each solution. The second column contains the values of the density, d, at 20° C., obtained by weighing a special form of pipette carefully filled with the solution. The third column gives w, the weight of $ZnSO_4$ in grams per cubic centimetre of solution, and is obtained by dividing the product of the numbers in the first two columns by 100. If we add to this weight w, the number 0·9982, representing the weight of water in grams per c.c. at 20° C., and subtract the observed density, d, of the solution, we obtain as the difference given in the fourth column, the weight of water displaced per c.c. by the zinc sulphate in solution.

The observations of Lannoy reduced on a similar plan are as follows :—

(p).	(d) at 15° C.	$pd/100$.	Difference 0·9992 + w − d.	Calculated.
2·25	1·0226	0·0230	−0·0004	0·0000
5·60	1·0596	0·0594	−0·0010	0·0000
11·21	1·1238	0·1250	+0·0004	0·0000
16·85	1·1949	0·2013	+0·0056	+0·0057

These observations, taken in conjunction with our own, would appear to indicate a simple relation between the density and the composition, of a kind which so far as we are aware has not been previously observed. Up to a density of about 1·150, the solution of zinc sulphate appears to take place approximately without change of volume. The added molecules of $ZnSO_4$ do not appear to displace any of the molecules of water, so that the density at 20° C. is very nearly 0·9982 + w. Beyond this point, it appears that each added molecule of $ZnSO_4$ displaces one molecule of water, so that the density of the solution is very approximately represented by the expression

$$d = 0.9982 + w - 18(w - 0.150)/161 \quad \ldots \ldots \quad \text{(D)}.$$

The nature of this relation is perhaps more clearly shown by the curves given in fig. 3. In this figure, the values of w are taken as abscissæ, and the corresponding values of the difference 0·9982 + w − d, as ordinates. The sharp break which occurs at the point $w = 0.150$ is very clearly shown both by the observations of Lannoy, which are represented by crosses, and by our own which are represented by the dots enclosed in circles. Those of Lannoy unfortunately do not extend far enough to afford a satisfactory verification throughout the range, but we have no reason to distrust our

own observations at the higher points, as they were all carefully verified. The dotted curve, which is practically a straight line, represents the formula of Bender and Valson, which smooths out the break.

It is not theoretically improbable that a simple relation of this type should be found to hold approximately in the case of salt solutions. At the same time it is hardly to be expected that any such simple expression should represent *accurately* the changes of density at all temperatures. The expansion of water is anomalous in the neighbourhood of the freezing point, and the coefficients of expansion of solutions differ considerably from that of water at low temperatures, and generally increase with increase of strength of solution. These variations in the coefficients of expansion may well introduce secondary effects of a corresponding order in the changes of density.

In comparing the observations of Lannoy with our own, which were taken at a slightly different temperature, it would appear not improbable that systematic differences of this kind may exist, but the point obviously requires much more careful investigation, as the differences shown are so small, and might readily be explained by errors of observation. For instance, at the two lowest points the density according to Lannoy is greater than $0.9992 + w$. Since he apparently determined the composition of the solution by weighing out quantities of the heptahydrate, the discrepancy might be explained by a slight degree of efflorescence of the sample used for these determinations. In the table of densities given by Carhart, the composition of the solution in each case is stated in terms of the percentage of $ZnSO_4$ by weight in 100 parts of solution. If we assume, in the absence of any definite statement, that the symbol $ZnSO_4$ stands in this case also for the heptahydrate, we find that the densities which he gives are much greater than those found by Lannoy, or by ourselves, the value of the density at 45 per cent. of the heptahydrate, according to Carhart, being 1.343, instead of 1.318, as given by our observations. It is possible that the sample used in this case may have consisted largely of the hexahydrate, or the discrepancy may be due to other causes.

If we take older determinations of the density of zinc sulphate solutions, such as those of Gerlach or of Schiff (1859), we find that they show a general agreement with our observations rather than with the formula of Bender, but that the characteristic point to which we have drawn attention is neatly smoothed out in the tables which they give as deduced from the results of their observations. The point in question would not be noticed at all unless the observations were plotted by the method of differences, as shown in Fig. 3. and even in that case it might readily be mistaken for an error of observation, unless the points were numerous, and had been inde-

pendently checked. If we had ourselves foreseen a simple relation of this character, we should have taken even greater pains in verifying the observations about this point. As it is, we hope shortly to be able to investigate the subject further, and in particular to endeavour to find similar relations in the case of other solutions.

FIG. 3.

Abscissae. Values of $w = pd/100$.
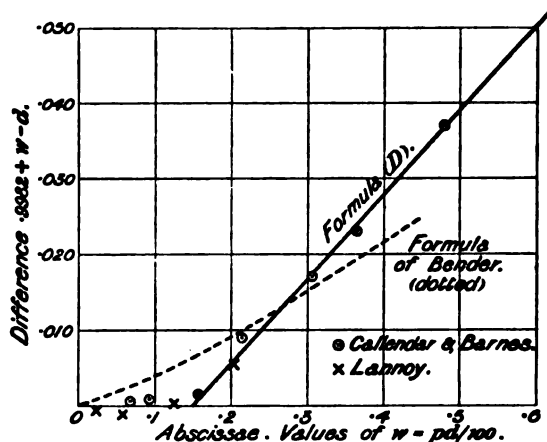
§ 22. *Change of E.M.F. with Strength of Solution at Constant Temperature.*

A number of Clark cells of the B.O.T. pattern, but without crystals, were set up with the solutions above described, of which the density and composition had been carefully determined. Due precautions were taken in each case to avoid evaporation. The difference of E.M.F. from the standard at 15° C., and also at other temperatures, was carefully determined in the case of each of these cells. On plotting the results, we could not find any simple relation between the change of E.M.F. and the density or the percentage strength of the solutions. But on expressing the observations in terms of w, the weight of $ZnSO_4$ per c.c., and not per gram, of solution, we found that the values of dE, the difference of E.M.F. from the standard at 15° C., fell very nearly on a straight line, represented by the formula :—

$$dE = 42.0 - 88.0w \text{ (millivolts)}.$$

The following table contains the observations for each solution tested :—

Table III.—Change of E.M.F. with Strength of Solution at 15° C.

Type of cell.	(*w*) gram per c.c.	*d*E millivolts.	*d*E calculated.	Difference, obs. − calc.
B.O.T. unsaturated	0·105	33·2	32·8	+0·4
	0·116	31·6	31·8	−0·2
	0·199	24·6	24·5	+0·1
	0·263	19·1	18·9	+0·2
Sat. at 0° C. ...	0·401	6·5	6·7	−0·2
Sat. at 15° ..	0·478	0·0	0·0	0·0

The differences given in the last column are of the same order as the accidental changes of E.M.F. observed in the case of these weaker cells. It would therefore appear probable that in this type of cell the diminution of E.M.F. is simply proportional to the volume concentration of the salt.

In comparing the above results, a curious point remains to be noticed. Taking a cell saturated at 15° C., the increase of E.M.F., on cooling down to 0° C., has been shown above to be 8·4 millivolts, if there is no change of strength of the solution. The increase of E.M.F., due to change of strength of solution from saturation at 15° C. to saturation at 0° C., has been found to be 6·5 millivolts. We might, therefore, naturally expect the total effect due to both causes combined to be 14·9 millivolts, whereas the saturated crystal cells, in which both causes are operative, show an increase of E.M.F. of 16·6 millivolts.

The explanation of this apparent discrepancy is to be found probably in the lowering of E.M.F., due to the greater diffusivity of the mercurous sulphate in the weaker solutions. In the saturated crystal cells this diffusion is practically prevented by the dense layer of crystals. In order to test this hypothesis, some weaker cells were set up in the W form, in which the possibilities of diffusion are diminished by the smallness of the tube and the increased distance between the electrodes. These cells showed, as was expected, higher values of the E.M.F. than those given by the formula, the difference amounting in some cases to between 2 and 3 millivolts.

§ 23. *On the Solubility of Zinc Sulphate.*

It is well known that zinc sulphate forms various hydrates, which may be obtained by crystallisation at different temperatures. These hydrates differ in point of solubility, and it is important for Clark cells to employ the heptahydrate, which has the lowest solubility at

temperatures between 0° and 30° C., and may therefore be considered the normal hydrate at these temperatures.

The peculiarities of the curve representing the temperature-variation of the E.M.F. of Clark cells are undoubtedly due in the main to the very considerable changes of solubility of this hydrate with rise of temperature, and are closely associated with the formation of different hydrates at higher temperatures. We therefore thought that it would be of interest to investigate this point more closely, at least within the range of temperature over which accurate measurements of the E.M.F. may be readily obtained.

The solubility of zinc sulphate has been studied by Étard,[*] who discovered that the solubility of the sulphates decreased with rise of temperature at higher points of the scale. His results are expressed in terms of the percentage weight, p, dissolved in 100 parts of the solution at a temperature $t°$ C. Expressed in this manner, he finds that the curve representing the solubility is a straight line, the equation of which for zinc sulphate is given as being

$$p = 27\text{·}6 + 0\text{·}2604t \quad \dots\dots\dots\dots\dots \quad \text{(Étard.)}$$

This equation is given as representing the solubility up to a temperature of about 80° C., above which the solubility decreases.

Roscoe and Schorlemmer,[†] on the other hand, give the following table of solubility expressed in terms of the weight of $ZnSO_4$ dissolved in 100 parts by weight of water. For the sake of comparison, we have reduced their results to the corresponding percentage, p, of solution, and have added two lines of results calculated from the formula of Étard, and from the observations of Poggiale.

Table IV.—Solubility of Zinc Sulphate (Roscoe and Schorlemmer, Étard and Poggiale).

Temperature C. .... $ZnSO_4$ in 100 of water (R. & S.).	0° 41·3	20° 53·0	50° 66·9	75° 80·4
Percentage of { R. & S. .... Solution (p) { Étard .... { Poggiale	29·2 27·6 30·0	34·6 32·8 34·7	40·1 40·6 40·7	44·5 47·1 45·0

It is evident from the discrepancies shown in the last three lines that the matter requires further investigation. From our own experiments we find that the rate of diffusion in these extremely dense and viscous solutions is so slow that it is much more difficult

[*] 'Compt. Rend.,' vol. 106 (1888), p. 206.
[†] Vol. 2, Part I, p. 262.

to obtain correct results than might at first sight be expected. In particular, we find it extremely important to maintain the solution for a considerable time with continual stirring at each point at a perfectly constant temperature. This essential condition could be readily secured by the aid of the constant temperature baths already described.

The method which we adopted at each temperature was to prepare a saturated solution with repeated stirring in a large test-tube in the constant temperature bath. This solution, mixed with crystals, was maintained for several hours at a constant temperature. Samples of solution free from crystals were drawn off in a special pipette at intervals and weighed. They were then evaporated to dryness at 100° C., and the percentage of $ZnSO_4$ in each case was calculated, assuming the residue to be the monohydrate. The different samples at each temperature always agreed very closely, showing the solution to have been saturated and free from crystals.

The results of our observations are contained in the following table. With the exception of the last four lines, which represent single observations, each line is the mean of two or three determinations. Observations in different lines were taken on different days and with different samples of solution.

Table V.—Solubility of Zinc Sulphate (Callendar and Barnes).

Temperature centigrade. (t).	Percentage of solution. (p).	Calculated by formula $29\cdot5 + 0\cdot270t$.	Difference (see Fig. IV). Obs.—Calc.
0°	29·43	29·50	−0·07
0	29·53	29·50	+0·03
0	29·49	29·50	−0·01
15·00°	33·66	33·55	+0·11
15·88	33·85	33·78	+0·07
30·70	38·46	37·80	+0·66
39·92	41·36	40·28	+1·08
39·95	41·37*	40·29	+1·08
40·73	41·43	40·50	+0·93
41·49	41·70	40·70	+1·00
46·40	42·68	42·02	+0·66
49·97	43·51*	43·00	+0·51
49·99	43·41	43·00	+0·41
50·00	43·50*	43·00	+0·50
50·20	43·51	43·05	+0·46

The observations marked with an asterisk were obtained by heating the solution to a temperature ten or twenty degrees above that of the bath and then allowing it to cool down to the bath temperature with constant stirring.

In order to exhibit the nature of these results on a convenient scale, it is necessary to plot, not the whole percentage, as is usually the case, but the small differences from the linear formula given in the fourth column. It is then apparent that there is a singular point in the curve of solubility at a temperature of 39° C. At this point, as the temperature rises, the rapidly increasing solubility of the heptahydrate begins to exceed that of the hexahydrate, which is also increasing, but less rapidly. At temperatures above 39°, a solution containing only crystals of the heptahydrate becomes supersaturated with respect to the hexahydrate, so that if any crystals of the latter are formed or introduced, there will be rapid crystallisation, and the strength of the solution will diminish. At 39° C. the solubilities of the two hydrates are equal and amount to 41·1 per cent. of the solution; but whereas the rate of increase of the solubility of the heptahydrate is 0·33 per cent. per 1° C., that of the hexahydrate is only 0·22 per cent. per 1°.

The crosses marked R. and S. in fig. 4, at the points 0° and 20°, correspond to the values given by Roscoe and Schorlemmer plotted on the same scale. The remaining observations of Roscoe and Schorlemmer, and also those of Étard, differ so much from ours that

Fig. 4.
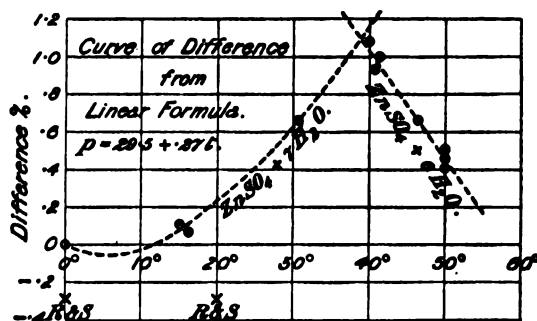
they could not be included in the limits of the page. The observations of Poggiale do not show the diminution of solubility above 80° C. which Étard gives. There is a general agreement in the results quoted, especially between those of Roscoe and Schorlemmer and Poggiale, but if the differences may be taken as indication of the order of accuracy attained, it is evident that the results of these observers could not be expected to show the critical point at 39° C.

By interpolation on the curve given in fig. 4, we find the following values for the solubility as given by our observations.

Temperature C.	0°	10°	20°	30°	40°	50°
Per cent. of sol. (*p*) ..	29·50	32·24	35·13	38·22	41·33	43·45
ZnSO₄ in 100 aq.	41·85	47·58	54·16	61·86	70·44	76·84
Compare Poggiale* ...	43·02	48·36	53·13	58·40	63·52	68·75
Difference	−1·17	−0·78	+1·03	+3·46	+6·92	+8·09

§ 24. *Change of E.M.F. at Higher Temperatures.* (*Between* 30° *and* 50° C.)

Finding that there was a change in the continuity of the curve representing the temperature variation of the E.M.F. at temperatures above 41° C., we decided to investigate this point more closely, although the temperatures in question lie beyond the range of the practical use of Clark cells. By a slight modification of the heating arrangements, the constant temperature baths were enabled to reach steady temperatures up to and beyond 50° C. The following series of observations were taken with several cells of different types, in a manner similar to that which has been already described. Each line represents the mean of the different cells at each point. The observations given in different lines were taken on different days, in the order in which they are given in the table. An interval of several months intervened between the first and the last half of the table.

Table VI.—Change of E.M.F. between 30° and 50° C.

Temperature centigrade.	Difference in millivolts from 15° C.		Difference from linear formula (L).
	Observed.	Formula (L).	
40·60°	35·81	30·72	5·09
30·14	19·78	18·17	1·61
30·32	20·00	18·38	1·62
35·44	27·39	24·53	2·86
42·58	36·56	33·10	3·46
46·74	41·14	38·09	3·05
48·58	43·50	40·30	3·20
35·79	28·08	24·95	3·13
40·09	34·99	30·11	4·88
42·79	36·76	33·35	3·41
44·70	38·88	35·64	3·24
41·54	35·45	31·85	3·60

* 'Watt's Dictionary of Chemistry.' Muir and Morley, vol. 4, p. 581.

The differences from the linear formula given in the last column of Table VI are shown by the crosses on the curve in fig. 5. This figure is plotted, as in the previous case, to exhibit not the whole change of the E.M.F., but only the defect of the change from lineality. For instance, at 39° C., the E.M.F. does not suddenly begin to increase, but continues to diminish at a slower rate.

It was evident on plotting these observations that there was a break in the curve, and that the observations after 41° C. belonged to a different branch. To investigate this point more closely, a continuous set of readings was taken on a pair of very sensitive cells. Starting at a temperature of 35° C., the temperature of the bath was

FIG. 5.
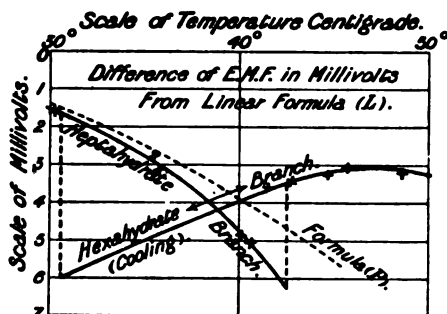
very slowly and continuously raised by disconnecting the regulator, and readings were taken of the temperature and E.M.F. of the cells alternately every few minutes. The E.M.F. of the cells was observed to fall at a steady rate, accurately following the curve already found for the observations at steady temperatures, until a temperature of 42·4° was reached. At this point there was a sudden increase of more than 2 millivolts in the E.M.F., due to rapid crystallisation of the hexahydrate, and in less than five minutes the cells had reached a point on the other branch of the curve.

After remaining at this point for some hours, the bath was slowly and continuously cooled, observations being taken in the same manner during the cooling. As had been expected, the cells were found to follow the branch of the curve shown in fig. 5, corresponding to the solubility of the hexahydrate. The solution remained supersaturated with respect to the heptahydrate until a temperature of 31° was reached, at which point the E.M.F. had fallen nearly five millivolts below the normal. At this point there was a sudden crystallisation of the heptahydrate, and the E.M.F. rose in a few minutes to its normal value. It will be observed that the two branches of the curve

cross at 38·8°, which is in practical agreement with the temperature
of equal solubility of the two hydrates, as determined by the observa-
tions on the change of solubility.

§ 25. *Permanence and Reproducibility of B.O.T. Crystal Cells.*

It is now more than a year since the completion of the experiments
described in the preceding pages, and we are able to add the results
of more recent comparisons of the cells as evidence of the order of
their permanence and reproducibility. Of the original crystal cells,
made more than two years ago, we still have a few remaining. None
of the cells of this type have shown any signs of failure, in spite of
the treatment to which they have been subjected, but many of them
have been taken away by the students who made them.

Table VII.—Comparisons of Crystal Cells at 15° C.

Number of cell.	Differences from mean of cells in millivolts.				
	Dec. 10, 1895.	Nov. 28, 1896.	Dec. 19, 1896.	Feb. 8, 1897.	Aug. 2, 1897.
X 1	−0·03	−0·04	−0·08	−0·02	−0·04
X 2	−0·05	+0·02	+0·06	−0·04	+0·02
X 3	+0·10	+0·04	+0·05	..	+0·12
X 5	+0·03	−0·08	−0·07	+0·02	−0·12
X 6	+0·08	+0·07	+0·08	+0·04	
X 10	−0·07	+0·04	+0·01	..	+0·10
X 11	−0·08	−0·06	−0·07	..	−0·02

At 20° C.

As there is no particular reason why these cells should be less
permanent than other Clark cells of the B.O.T. type, the above will
probably be sufficient proof of permanence. It will be observed that
the average difference from the mean in each case is nearly one-
twentieth of a millivolt. The extreme difference is one-tenth. We
have observed that this is about the order of agreement generally
attainable with Clark cells set up at different times.

Over comparatively short intervals of time, such as one month, it
would appear from the above list, and from other tests, that the
average change in the value of any one cell, as compared with the
mean, may be expected not to exceed two or three hundredths of a
millivolt, but for longer periods, such as a year, the mean change
reaches one-twentieth.

As a further illustration of the reproducibility of these cells, and
of the close agreement in the temperature variation of the E.M.F.,
under somewhat exacting conditions of testing, we add a list of the

cells made by the fourth-year class of electrical students during the session of 1896–1897. These cells were all of the portable B.O.T. form, with a flattened and amalgamated platinum wire in place of mercury. They were set up in test-tubes, filled with crystals, and sealed with marine glue, and were otherwise exactly similar to the crystal cells described in § 10.

With the exception of those marked with an asterisk, the observations were all taken by the students themselves in the course of an afternoon's work. Readings were taken at the points 15°, 0°, 15°, 30°, 15°, allowing only about half an hour at each temperature.

Table VIII.—Portable Crystal Cells, made by Students.

Name of student.	Date when made.	Date when tested.	Difference from standard at 15°.	Change of E.M.F.		Mean change per degree.
				0°—15°.	15°—30°.	
Stovel..........	Mar. 2 ..	Mar. 4 ..	−0·17	16·57	19·39*	1·199
Thomson	Mar. 2 ..	Mar. 4..	−0·11	16·60	19·38	1·199
Blair	Feb. 19 .	Feb. 21 .	+0·05	16·63	19·41	1·201
Burnham	Dec. 8 ..	Jan. 19 .	−0·02	16·57	19·30*	1·196
Davidson	Jan. 12..	Jan. 19 .	+0·11	16·62	19·40	1·201
Edwards	Dec. 10 .	Jan. 26 .	+0·07	16·60*	19·36*	1·199
Macbean	Dec. 3 ..	Dec. 8 ..	−0·04	16·62	19·36	1·199
McDonald, P. W.	Jan. 14	Jan. 19 .	+0·14	16·63	19·49	1·204
McDonald, J. E.	Feb. 19 .	Feb. 21 .	−0·15	16·58	19·30	1·196
Packard........	Jan. 12	Jan. 19 .	+0·14	16·56	19·43	1·200
Pitcher	Dec. 10 .	Jan. 26 .	+0·05	16·58*	19·53	1·204
Walters	Dec. 3 ..	Dec. 8 .	−0·13	16·61	19·44	1·202
Mean of students' cells			−0·005	16·603	19·400	1·2000

It should be observed that the cells were, in most cases, tested rather too soon after being sealed up. In the course of a week or two, they were usually found to have settled down into closer agreement with the standard. When kept for a longer time than half an hour at 30° C., they showed a slightly greater change of E.M.F. at this point.

November 18, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Dr. J. S. Haldane, Mr. George Murray, Professor H. Alleyne Nicholson, and Professor H. H. Turner were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President reported that during the recess, at a Court held at Windsor on July 15, 1897, he, accompanied by Sir J. Evans, Treasurer; Professor M. Foster, Secretary; Professor A. W. Rücker, Secretary; Professor R. B. Clifton, Sir W. Huggins, and Mr. W. T. Thiselton-Dyer, Vice-Presidents; Sir J. D. Hooker, Lord Kelvin, and Sir G. G. Stokes, Past-Presidents, had the privilege of being admitted to the presence of the Throne and the honour of presenting to Her Gracious Majesty the loyal Address which had been approved by the Fellows at their meeting of May 20, and that Her Majesty in accepting the Address made a gracious reply.

The Address and Royal Reply are as follows :—

" *To the Queen's Most Excellent Majesty.*

" May it please your Majesty !

" We, your loyal and dutiful subjects, the President, Council, and Fellows of the Royal Society of London, humbly beg leave to offer to your Most Gracious Majesty, the beloved Patron of our Society, our respectful and sincere congratulations on the happy conclusion of the sixtieth year of your Most Gracious Majesty's reign, a reign which has extended over a longer period than has that of any of your Majesty's illustrious Predecessors. Not in the number of years only has your Majesty's reign surpassed all others : it has also been marked by unexampled prosperity and progress. Conspicuous in that progress, and an acknowledged cause of that prosperity, has been the marvellous advancement during your Majesty's reign of that Natural Knowledge to promote which your Majesty's illustrious Predecessor, King Charles II, founded the Royal Society. Your humble and devoted servants, the Fellows of the Royal Society, feel that they, in common with all men of science, have especial reason to rejoice in the long continuance of your Majesty's beneficent rule.

" We beg your Majesty graciously to receive this token of the loyal devotion which the men of science of this country feel towards their Sovereign Lady the Queen.

"And we shall ever pray that your Most Gracious Majesty may be spared for many years to reign over your dutiful and loving people."

Her Majesty's Reply.

"I thank you for your loyal and dutiful Address. I am much gratified by the attachment which your ancient and learned Society expresses to My Throne and Person.

I am fully sensible how far the labours and ingenuity of men of science, whom you worthily represent, have advanced the industrial and social prosperity of My people, and have tended alike to their good and refinement, and I confidently expect the same excellent fruit in years to come from the indefatigable and reverent Investigation of Nature for the promotion of which the Royal Society was founded."

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Mr. Horace Brown, Sir Douglas Galton, and Dr. T. E. Thorpe were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The Secretary read the following list of Papers received since the last meeting and published in accordance with the Standing Orders :—

Professor J. A. FLEMING and Professor J. DEWAR. Further Observations on the Dielectric Constants of Frozen Electrolytes at and above the Temperature of Liquid Air.

Professor J. DEWAR and Professor J. A. FLEMING. On the Dielectric Constants of Certain Organic Bodies at and below the Temperature of Liquid Air.

Professor J. DEWAR and Professor J. A. FLEMING. On the Dielectric Constants of Metallic Oxides dissolved or suspended in Ice cooled to the Temperature of Liquid Air.

Professor RUBERT BOYCE and Professor W. A. HERDMAN. On a Green Leucocytosis in Oysters associated with the Presence of Copper in the Leucocytes.

CHARLES S. TOMES. On the Development of Marsupial and other Tubular Enamels, with Notes upon the Development of Enamel in general.

Dr. MOND, Professor RAMSAY, and Dr. J. SHIELDS. On the Occlusion
of Oxygen and Hydrogen by Platinum Black. Part II.
J. E. MURRAY. On Contact Electricity of Metals.
W. GARDINER. The Histology of the Cell Wall, with Special Refer-
ence to the Mode of Connection of Cells.
H. L. CALLENDAR and H. T. BARNES. On the Variation of the Elec-
tromotive Force of Different Forms of the Clark Standard Cell
with Temperature and with Strength of Solution.
Lord RAYLEIGH. On the Viscosity of Hydrogen as affected by
Moisture.

The following Papers were read :—

I. " Account of a Comparison of Magnetic Instruments at Kew
Observatory." By C. CHREE, Sc.D., F.R.S., Superintendent.

II. "Note on the Influence of very Low Temperatures on the Germi-
native Power of Seeds." By HORACE T. BROWN, F.R.S., and
F. ESCOMBE, B.Sc., F.L.S.

III. " On the Structure and Affinities of Fossil Plants from the
Palæozoic Rocks. II. On *Spencerites,* a new Genus of
Lycopodiaceous Cones from the Coal-measures, founded on
the *Lepidodendron Spenceri* of Williamson." By D. H.
SCOTT, M.A., Ph.D., F.R.S., Hon. Keeper of the Jodrell
Laboratory, Royal Gardens, Kew.

IV. "Antagonistic Muscles and Reciprocal Innervation. Fourth
Note." By C. S. SHERRINGTON, F.R.S., and Dr. E. H. HERING.

At the request of the President, Mr. Gardiner made an oral state-
ment on the subject of his Paper published during the vacation (see
list above).

" Account of a Comparison of Magnetic Instruments at Kew
Observatory." By C. CHREE, Sc.D., F.R.S., Superintendent.
Received October 26,—Read November 18, 1897.

Last July, M. T. Moureaux, of the Parc Saint-Maur Observatory,
near Paris, brought over to England the travelling instruments em-
ployed in his magnetic survey of France, and a comparison was made
between these and the standard magnetic instruments at Kew
Observatory. At the expressed desire of the Kew Observatory Com-
mittee, I submit on their behalf a brief account of the comparison
and its results.
The comparison serves to connect the standard instruments at
Kew Observatory with the standard French instruments at Parc
Saint-Maur, the latter, as M. Moureaux has had the goodness to

inform me, being in excellent agreement with his travelling instruments. Parc Saint-Maur may be regarded as the base station for M. Moureaux's great survey of France and Algeria, while Kew Observatory performed a similar function in the surveys of Great Britain and Ireland, by Professor Rücker and Dr. Thorpe. The existence of the English Channel introduces uncertainty into any conclusions based on the trend of the magnetic lines in France and England, and the instruments employed in the two countries are sufficiently dissimilar to justify scepticism as to their close agreement in the absence of direct experiment. The interest of the comparison is thus far from being limited to the two observatories most immediately concerned.

M. Moureaux's observations at Kew Observatory occupied the afternoon of July 26, and the forenoons of July 27, 28, and 29. On the afternoons of the last three days, observations were made with the Kew standard instruments, by Mr. T. W. Baker, chief assistant at the Observatory. All the observations were made in the "magnetic house" in the Observatory garden.

The comparison was really between M. Moureaux's absolute instruments and the Kew absolute instruments, but the observations, being made at different hours of the day, had to be connected through the intermediary of the curves from the self-recording magnetic instruments. The elements recorded photographically are the declination, horizontal force, and vertical force. The value in magnetic units of 1 cm. of the ordinates is known, but the value of the base lines, answering to zero ordinates, of the several curves is to a certain extent variable. The usual practice at Kew Observatory is to treat each month separately, deducing the value of the base line for any element from a comparison of the absolute observations for that month with the curve ordinates at the times of the observations.

In the case of the declination and horizontal force, the standardization of the curves is comparatively simple. In the case of the vertical force, the influence of temperature is unfortunately somewhat large, a rise of $1°$ F. equalling in effect a fall of $0·0001$ C.G.S. unit in the vertical component. There is also the complication that what the curve gives is the vertical force, while what the absolute instrument gives is the inclination.

Thus to compare inclinometers used at different hours, one has to follow a circuitous route by way of the horizontal and vertical components, allowing a correction for changes of temperature in the magnetograph room during the observations.

M. Moureaux observed the inclination early, and Mr. Baker late, in the day, and there happened to be a slight difference in the mean temperature of the magnetograph room at the times of their observations.

Taking into consideration the above facts, and the further fact that M. Moureanx's visit occurred at the end of a month, it was decided to standardise the curves exclusively from Mr. Baker's special observations, on July 27 to 29. These gave three or more complete determinations of each element, under conditions which might be described, on the evidence of the curves, as an almost perfect magnetic calm.

Mr. Baker's absolute observations and the corresponding curve measurements were in good agreement, especially in the case of the horizontal force, where the individual calculated values for the base line of the curves showed no difference greater than 0·00002 C.G.S. unit.

Owing to the less direct method of comparing the inclinometers, I regard the results obtained for them as somewhat less trustworthy than the others.

The figures under the heading " Observatory—Moureaux " are to be regarded as the excess in the readings of the absolute Kew instruments over those of M. Moureaux's instruments, supposing the former to have been read simultaneously with the latter. The times specified are actually those occupied by M. Moureaux's observations.

Declination.

Date.	Time.	Kew Observatory.	Moureaux.	Observatory—Moureaux.
July 26	3.47— 4.2 P.M.	17° 5·9′	17° 5·7′	+0·2′
27	10.5 —10.18 A.M.	4·8	5·0	−0·2
27	10.22—10.32 „	6·0	5·7	+0·3
28	9.9 — 9.24 „	2·1	1·3	+0·8
28	9.28— 9.40 „	3·0	1·9	+1·1
29	11.37—11.49 „	9·9	9·3	+0·6
			Mean......	+0·5′

Horizontal Force.*

Date.	Time.	Kew Observatory. C.G.S.	Moureaux. C.G.S.	Observatory—Moureaux. (Unit being 10⁻⁵ C.G.S. unit).
July 26	4.13— 4.42 P.M.	0·18354	0·18356	− 2
27	10.41—11.10 A.M.	25	24	+ 1
28	9.48—10.18 „	28	49	−21
28	10.43—11.11 „	20	39	−19
29	10.24—10.56 „	20	43	−23
29	11.1 —11.31 „	20	27	− 7
			Mean....	−0·00012 C.G.S. unit.

* [Nov. 15, 1897. M. Moureaux requests me to explain that in the present comparison of horizontal force at Kew—as well as in his recent comparisons at

Inclination.

Date.	Time.	Kew Observatory.	Moureaux.	Observatory— Moureaux.
July 27	11.15—11.41 A.M.	67° 20·2′	67° 18·9′	+1·3′
28	11.21—11.43 ,,	20·3	18·7	+1·6
29	8.55— 9.18 ,,	19·8	17·3	+2·5
29	9.21— 9.43 ,,	20·0	17·6	+2·4
29	9.47—10.9 ,,	20·0	18·0	+2·0
			Mean......	+2·0′

In judging of the results several things merit consideration. Neither inclinometer read to less than 1′, decimals arising from arithmetical operations. The Kew unifilar magnetometer reads to 10″, but with M. Moureaux's much smaller instrument readings could not be taken to less than 30″. The great skill of the two observers is beyond question, and the mean of several results obtained without mental bias may possess an accuracy greater than that which any individual reading can lay claim to. Still, personally, I should be very sorry to claim accuracy of the order of 1 in the last significant figure of the mean differences.

In the use of the results, one should remember the possibility, or rather probability, of the occurrence of change in magnetic instruments. This is a vicissitude to which travelling instruments are probably most exposed, but even in an observatory standard it is certainly not impossible. The constant "P," appearing in the factor $1 - \mathrm{P}r^{-2}$, which allows for the departure of the horizontal force magnet from the ideal infinitely short magnet, appears to be to some extent variable, at least in the Kew instrument. This particular variation does not necessarily affect the calculated horizontal force, because the proper value of " P " for a special set of observations can be calculated from the experiments themselves. This precaution was, in reality, actually adopted in the present case. Still the fact that a change does take place, which, if undetected, would appreciably influence the results, shows that assumptions of absolute constancy are at present acts of faith, not reason. Until there exists a regular system of intercomparison of the instruments at different observatories, our information on this head is likely to be limited.

Pavlovsk (St. Petersburg) and Uccle (Brussels)—he has made use of new values for the constants of the French instruments, which it is intended to apply consistently after Jan. 1, 1898.

This change, entailing a reduction of 0·00067 C.G.S. unit in the corrected readings of the French standard, was postponed until the French survey should be completed, though the necessity for it has been recognised for some years. Particulars will be found in a paper by M. Moureaux in the 'Annales du Bureau Central Météorologique de France,' vol. 1, 1896, now in the press.]

After this remark, I think I may safely utilise the present comparison to extend a table,* in which Professor Rücker and Mr. W. Watson embodied the results of their comparison of the standard instruments at various English and Irish observatories, made by means of travelling instruments, in 1895. The differences between the declination, horizontal force, and inclination instruments are given in order, one below the other, the unit in the case of the horizontal force being 0·00001 C.G.S. unit.

The table is to be read as follows :—" Standard declinometer at Kew reads higher than that at Parc Saint-Maur by 0·5', but lower than that at Falmouth by 0·8'," and so on. The last column gives the differences from the mean instrument, so to speak, of the five observatories.

	Kew.	Parc Saint-Maur.	Falmouth.	Stonyhurst.	Valencia.	Mean.
Kew Observatory	—	+ 0·5'	− 0·8'	+ 1·1'	0·0'	+ 0·2'
	—	−12	−18	− 6	+29	− 1
	—	+ 2·0'	− 1·6'	+ 2·2'	− 1·8'	+ 0·2'
Parc Saint-Maur	− 0·5'	—	− 1·3'	+ 0·6'	− 0·5'	− 0·3'
	+12	—	− 6	+ 6	+41	+11
	− 2·0'	—	− 3·6'	+ 0·2'	− 3·8'	− 1·8'
Falmouth	+ 0·8'	+ 1·3'	—	+ 1·9'	+ 0·8'	+ 1·0'
	+18	+ 6	—	+12	+47	+17
	+ 1·6'	+ 3·6'	—	+ 3·8'	− 0·2'	+ 1·8'
Stonyhurst	− 1·1'	− 0·6'	− 1·9'	—	− 1·1'	− 0·9'
	+ 6	− 6	−12	—	+35	+ 5
	− 2·2'	− 0·2'	− 3·8'	—	− 4·0'	− 2·0'
Valencia (Cahirciveen)	0·0'	+ 0·5'	− 0·8'	+ 1·1'	—	+ 0·2'
	−29	−41	−47	−35	—	−30
	+ 1·8'	+ 3·8'	+ 0·2'	+ 4·0'	—	+ 2·0'

The apparent agreement between the standard instruments at Parc Saint-Maur and Stonyhurst is noteworthy. The fact that the Kew standard instruments agree so closely with the imaginary mean instruments was, it may be observed, not noticed by the writer until after he had constructed the table. In his opinion the phenomenon is probably purely fortuitous. In searching, however, for explanations of the discrepancies between the several instruments. or in attempting to remove them, a consideration of the departures from the means might be profitable.

* 'Brit. Assoc. Report' for 1896, p. 97.

"Note on the Influence of very Low Temperatures on the
Germinative Power of Seeds." By HORACE T. BROWN,
F.R.S., and F. ESCOMBE, B.Sc., F.L.S. Received September
27,—Read November 18, 1897.

A considerable difference of opinion still exists amongst biologists
as to the condition of the protoplasts of resting seeds, spores, &c., in
which all ordinary signs of life may be unrecognisable for a consider-
able period.

According to one view, the essential elements of the cell, during
the period of inertness, are still undergoing feeble but imperceptible
alteration, accompanied by gaseous exchange with the surrounding
atmosphere; and, even when ordinary respiration is in abeyance, it is
assumed there are small internal changes going on, due to the inter-
action of certain constituents of the protoplasm, reactions which may
be independent of the outside gaseous medium, and which are often
referred to under the somewhat vague term of " intramolecular
respiration."

On the other hand, it is sometimes maintained that all metabolism
is completely arrested in protoplasm when in the dormant state, and
that it then loses, for the time being, all power of internal adjust-
ment to external conditions.

According to one view, therefore, the machinery of the dormant
protoplasts is merely " slowed down " to an indefinite extent, whilst
according to the other it is completely brought to rest for a time,
to be once more set going when external conditions are favourable.

It appears to us that the advocates of the " slowing-down " hypo-
thesis have scarcely given sufficient attention to the experimental
evidence available, and that they have been somewhat biassed by a
supposed analogy between the dormant state of seeds and the hiber-
nating state in animals, and have also, perhaps, been unconsciously
influenced by Mr. Herbert Spencer's well-known definition of life,
which implies a constant internal adjustment in the living protoplasm.

The experiments of the late G. J. Romanes, which were described
in a short paper laid before the Society in 1893[*] are full of interest
in their bearing on this question. Seeds of various plants were sub-
mitted in glass tubes to high vacua of 1/1000000 of an atmosphere for
a period of fifteen months. In some cases, after the seeds had been
in vacuo for three months, they were transferred to other tubes
charged respectively with oxygen, hydrogen, nitrogen, carbon
monoxide, carbon dioxide, hydrogen sulphide, aqueous vapour, and
the vapour of ether and chloroform. The results proved that neither
a high vacuum, nor subsequent exposure for twelve months to any of

* ' Roy. Soc. Proc.,' vol. 57, p. 335.

the above gases or vapours, exercised much, if any, effect on the subsequent germinative power of the seeds employed.

These experiments of Romanes are certainly of the highest importance, since the seeds were submitted for a long period to conditions which must certainly have excluded anything like respiration by ordinary gaseous exchange, but the conditions did not preclude with the same certainty the possibility of chemical interactions of some kind or other within the protoplasm, those ill-understood changes, in fact, which have been referred to " intramolecular respiration." It is true that in some of the experiments, notably those in which the vapours of chloroform and ether were employed, the probability of any such internal reactions is rendered somewhat remote, but still, in most cases, the experiments admit of the possibility of feeble metabolic activity continuing in the cytoplasm.

It occurred to us, some months ago, that more evidence would probably be forthcoming on these points if we could submit seeds to a temperature below that at which ordinary chemical reactions take place, thus eliminating any possibility of interactions between the constituents of the protoplasm.

Owing to the kindness of Professor Dewar, who has been good enough to place the resources of his laboratory at our disposal, and to undertake this part of the work for us, we have been able to ascertain how far the subsequent germinative power of a considerable variety of seeds is affected by prolonged exposure to the very low temperatures produced by the slow evaporation of *liquid air.*

The seeds, enclosed in thin glass tubes, were slowly cooled, and immersed in a vacuum-jacketted flask containing about 2 litres of the liquid air, which was kept replenished so as to submit the seeds for 110 consecutive hours to a temperature of from $-183°$ C. to $-192°$ C. About 10 litres of liquid air were required for the experiment.

The seeds had been previously air-dried only, so contained from about 10 to 12 per cent. of natural moisture. After the above treatment they were slowly and carefully thawed, a process which occupied about 50 hours, and their germinative power was then compared with control experiments made on other portions of the seed which had not been treated in any way.

The seeds experimented on were as follows :—

Hordeum distichon.	*Trigonella fœnum-græcum.*
Avena sativa.	*Impatiens balsamina.*
Cucurbita Pepo.	*Helianthus annuus.*
Cyclanthera explodens.	*Heracleum villosum.*
Lotus Tetragonolobus.	*Convolvulus tricolor.*
Pisum elatius.	*Funkia sieboldiana.*

These include representatives of the following natural orders :—
Gramineæ, Cucurbitaceæ, Leguminoseæ, Geraniaceæ, Compositæ,
Umbelliferæ, Convolvulaceæ, and Liliaceæ.

Some of the seeds are endospermous, others non-endospermous,
and the reserve material consists in some cases of starch, and in
others of oil or of mucilage.

Their germinative power, after being submitted to the low tem-
perature, showed no appreciable difference from that of the controls,
and the resulting plants, which were in most cases grown to full
maturity, were equally healthy in the two cases.

In 1892 Professor Dewar and Professor McKendrick found that a
temperature of −182° C. continued for one hour is insufficient to
sterilise putrescent substances such as blood, milk, flesh, &c., and
that seeds would germinate after the action of a similar temperature
for the same period of time.[*]

When we commenced our experiments we were unaware that any
other observations of a similar nature had been made, but whilst they
were in progress our attention was drawn to an important memoir
by C. de Candolle,[†] in which the latent life of seeds is discussed in
the light of a number of low temperature experiments made princi-
pally by himself and R. Pictet, and described at intervals in the
Geneva 'Archives.'[‡] In the earlier experiments of C. de Candolle
and Pictet, made in 1879, temperatures of −39° C. to −80° C.
were employed, and these only from two to six hours, whilst Wart-
mann in 1881 exposed seeds for two hours to −110° C. without
effect. In 1884 Pictet found that an exposure of various kinds of Bacte-
riaceæ for three days to −70° C., and afterwards for a further period
of thirty-six hours to −120°, did not destroy their vitality, and in the
same year Pictet and C. de Candolle exposed seeds to −100° C. for
four days with the same result. Pictet, in 1893, further extended
his observations to various microbes and also to a large number of
seeds, and claims to have cooled them down without effect to nearly
−200° C., but he gives no details of the experiments, nor any
indication of the length of time during which the cooling lasted.
His conclusions, however, are that, since all chemical action is
annihilated at −100° C., life must be a manifestation of natural laws
of the same type as gravitation and weight.

In his memoir of 1895 (*loc. cit.*) C. de Candolle discusses very

* 'Roy. Inst. Proc.,' 1892, vol. 13, p. 699.

† 'Archives des Sci. Phys. et Nat.,' Geneva, 1895, vol. 33, p. 497.

‡ E. Wartman, 1860, 'Archives des Sci. Phys. et Nat.,' 1860, p. 277; C. de
Candolle and Pictet, 1879, *ibid.*, vol. 2, p. 354; *ibid.*, vol. 2, p. 629; E. Wart-
mann, 1881, *ibid.*, vol. 5, p. 340; R. Pictet, 1884, *ibid.*, vol. 11, p. 320; R. Pictet
and C. de Candolle, *ibid.*, p. 325; R. Pictet, 1893, *ibid.*, vol. 30, p. 293; C. de
Candolle, 1895, *ibid.*, vol. 33, p. 497.

fully whether we must regard the life of the resting seed as completely arrested for a time or merely temporarily slackened (*ralentie*), and he gives the results of some new experiments on seeds maintained at from −37° C. to −53° C. in the " snow-box " of a refrigerating machine for a period of 118 days. Most of the seeds resisted this treatment successfully, and the author concludes that after a sufficient interval of time has elapsed the protoplasm of the ripe seed passes into a state of complete inertness in which it is incapable either of respiration or assimilation, and that whilst in this condition it can support, without detriment to its subsequent revival, rapid and considerable lowering of temperature.

De Candolle then points out that if it really be a fact that the suspended life of a resting seed is in any way dependent on respiration, we might expect this to be shown by submitting seeds to a barometric vacuum for some time. He does not appear to have followed out this suggestion, and is apparently unaware of the direct experiments on this point carried out by Romanes two years previously; he argues, however, that if ordinary respiration is a factor of any importance, this may be determined by immersing the seeds in mercury for such a length of time as to ensure the complete consumption of the small amount of oxygen contained within their tissues. It was found that when seeds of *wheat*, and of *Lepidium sativum* were thus treated, for periods varying from one to three months, their power of germination was not sensibly affected.

Although these last described experiments of C. de Candolle go far to show that any considerable amount of respiration is unnecessary for the maintenance of potential life in the protoplasm of resting seeds, they are not inconsistent with the view that some minute amount of gaseous exchange may be going on during the whole course of the experiment at the expense of the oxygen contained in the seeds at the time of immersion in the mercury. The results would have been far more conclusive on this point if it had been shown that the gaseous oxygen originally contained in the seed tissue had been completely used up in an early stage of the experiment. The experiments of Romanes, however, conducted with high vacua and atmospheres of various gases, leave no room for doubt on this question, and we must consequently abandon all idea of the dormant state of resting seeds having any necessary dependence whatever on ordinary respiratory processes. Neither set of experiments, however, excludes the possibility of molecular interchanges in the protoplasm itself, such molecular transpositions in fact as those which can often be induced in living cells placed under anaërobic conditions, and which are all exothermic in character, and generally, but not necessarily, attended with the liberation of more or less CO_2. The great value of the low temperature experiments we have described

lies in the fact that such processes of autoxidation, and in fact any
conceivable internal chemical change in the protoplasts, are rendered
impossible at temperatures of −180° C. to −190° C., and that we
must consequently regard the protoplasm in resting seeds as existing
in an absolutely inert state, devoid of any trace of metabolic activity,
and yet conserving the potentiality of life. Such a state has been
admirably compared by C. de Candolle with that of an explosive
mixture, whose components can only react under determinate con-
ditions of temperature; as long as these conditions remain unfulfilled
the substances can remain in contact with each other for an indefinite
period without combining.

It appears to us that the occurrence of a state of complete chemical
inertness in protoplasm, without a necessary destruction of its
potential activity, must necessitate some modification in the current
ideas of the nature of life, for this inert state can scarcely be included
in Mr. Herbert Spencer's well-known definition, which implies a
continuous adjustment of internal to external relations.* The defi-
nition doubtless holds good for the ordinary *kinetic* state of proto-
plasm, but it is not sufficiently comprehensive to include protoplasm
in the *static* condition in which it undoubtedly exists in resting seeds
and spores. The definition becomes in fact one of " vital activity "
rather than of life.

As it is inconceivable that the maintenance of " potential vitality "
in seeds during the exposure of more than 100 hours to a temperature
of −180° to −190° C. can be in any way conditioned by, or correlated
with, even the feeblest continuance of metabolic activity, it becomes
difficult to see why there should be any time limit to the perfect
stability of protoplasm when once it has attained the resting state,
provided the low temperature is maintained; in other words an
immortality of the individual protoplasts is conceivable, of quite a
different kind from that potentiality for unending life which is
manifested by the fission of unicellular organisms, and with which
Weismann has rendered us familiar.

In what manner and to what extent " resting " protoplasm differs
from ordinary protoplasm we do not at present know, but there are
indications, notably those afforded by the resting state of desiccated

* The following passage from the ' First Principles ' (Section 25) clearly shows
that the author in constructing his definition had not anticipated the possibility of
a *living* organism attaining a state of absolutely stable equilibrium. " All vital
actions, considered not separately but in their *ensemble*, have for their final purpose
the balancing of certain outer processes by certain inner processes. There are
unceasing external forces tending to bring the matter of which organic bodies
consist into that state of stable equilibrium displayed by inorganic bodies; there
are internal forces by which this tendency is constantly antagonised, and the per-
petual changes which constitute life may be regarded as incidental to the mainten-
ance of the antagonism."

Rotifera, and also by some recent experiments of Van Eyck on dis-
continuous germination,* that ordinary protoplasts may, by suitable
treatment, be brought to the "resting" condition.

In 1871, Lord Kelvin, in his Presidential Address to the British
Association, threw out the suggestion that the origin of life as we
know it may have been extra-terrestrial, and due to the "moss-grown
fragments from the ruins of another world," which reached the
earth as meteorites.† That such fragments might circulate in the
intense cold of space for a perfectly indefinite period without pre-
judice to their freight of seeds or spores, is almost certain from the
facts we know about the maintenance of life by "resting" proto-
plasm; the difficulties in the way of accepting such a hypothesis
certainly do not lie in this direction.

We must express our thanks to Mr. Thiselton-Dyer and to
Dr. D. H. Scott, for the facilities they have given us in carrying out
these experiments in the Jodrell Laboratory.

Addendum.

After the completion of the above Note, our attention was called to a
recent investigation by M. R. Chodat, on the influence of low tempera-
tures on *Mucor mucedo*.‡ He found that a lowering of temperature for
several hours to −70° to −110° C. failed to kill young spores of the
mucor, and he adduces certain evidence, which is not, however, wholly
convincing, that even the mycelium itself, when cultivated on Agar-
Agar, and whilst in active growth, is able to resist the action of these
low temperatures. The author sums up his opinion as to the bearing
of his experiments on the nature of life in the following words :—
"La respiration elle-même est évidemment complètement arrêtée à
cette température où les corps chimiques ne réagissent plus les uns
sur les autres. Si l'on considère que la vie consiste principalement
en un échange continuel de substance, soit par la nutrition intra-
cellulaire, soit par la respiration, alors il faut convenir qu'à ces tem-
peratures basses la vie n'existe plus. C'est une fatale erreur qu'on
rencontre dans presque tous les traités que la respiration est une
condition nécessaire de la vie, alors qu'elle n'est qu'une des con-
ditions de sa manifestation. La vie est conditionnée par certaines
structures. Les forces qui les mettent en jeu peuvent être des forces
toutes physiques. Elles sont simplement les sources d'énergie qui
pourront mettre la machine en mouvement."

* 'Ann. Agron.,' vol. 21 (1895), p. 236.

† We find that Professor Dewar called attention in one of his Royal Institution
lectures in 1892 to the bearing of his low temperature experiments with spores,
&c., on this suggestion of Lord Kelvin's (see 'Roy. Inst. Proc.,' 1892, vol. 13,
p. 699).

‡ 'Bulletin de l'Herbier Boisier,' vol. 4 (1896), p. 894.

"On the Structure and Affinities of Fossil Plants from the Palæozoic Rocks.   II. On *Spencerites*, a new Genus of Lycopodiaceous Cones from the Coal-measures, founded on the *Lepidodendron Spenceri* of Williamson."   By D. H. SCOTT, M.A.. Ph.D., F.R.S., Hon. Keeper of the Jodrell Laboratory, Royal Gardens, Kew.   Received November 2, —Read November 18, 1897.

(Abstract.)

The fossils which form the subject of the present paper are Cryptogamic strobili, showing evident Lycopodiaceous affinities, but differing in important points from other fructifications of that family, so that it appears necessary to establish a new genus for their reception.

Two species are described, one of which (*Spencerites insignis*) is already known to us from the investigations of Williamson, who named it first *Lepidostrobus insignis*, and afterwards *Lepidodendron Spenceri*,[*] while the other (*Spencerites majusculus*) is new.

In one of his latest publications, Williamson pointed out that it might ultimately be necessary to make his *Lepidodendron Spenceri* the type of a new genus.[†]   The separation thus suggested is now carried out, on the basis of a renewed investigation of the structure of this fossil.

Spencerites insignis is a pedunculate strobilus; the vegetative organs are not as yet identified.   The specimens are calcified, and their structure admirably preserved.

The anatomy of the axis is of a simple Lycopodiaceous type, but differs in details (such as the course of the leaf-trace bundles) from that of the axis of *Lepidostrobus*.   The peduncle bears sterile bracts, similar to the sporophylls of the cone itself; the latter are arranged spirally, or in some cases in alternating verticils.

The individual sporophylls are of peltate form, consisting of a short cylindrical pedicel, expanding into a relatively large lamina. The sporangia are approximately spherical bodies; unlike those of *Lepidostrobus*, they are quite free from the pedicel, and are attached by a narrow base to the upper surface of the lamina, where it begins to expand.

The details of the sporangial wall are quite different from those of *Lepidostrobus*, and the spores are also characteristic.   In size they are intermediate between the microspores and macrospores of

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* Williamson, "Organization of the Fossil Plants of the Coal-measures," Parts IX, X, XVI, and XIX, 'Phil. Trans.,' 1878, 1880, 1889, and 1893.

† General Index, Part II, 1893, p. 24.

Lepidostrobus. They are of tetrahedral form, becoming spheroidal when mature, and each spore has a hollow, annular wing running round its equator. The wing is no doubt formed by a dilation of the cuticle,[*] and not, as Williamson supposed, from the abortive sister-cells.

Spencerites majusculus, the new species, is much larger than the former, the axis of the cone being twice as thick. The anatomy is similar, but the sporophylls, and consequently the leaf-traces, are more numerous. The sporophylls, which are arranged in alternating verticils, are relatively short, and of peculiar form; the lamina is very thick, and of great tangential width. The sporangia are like those of the former species, and similarly inserted, but the spores are quite different. They are smaller than those of *S. insignis,* and have the form of quadrants of a sphere, with narrow wings along their three angles.

The genus is separated from *Lepidostrobus,* mainly on account of the very different mode of insertion of the sporangia, a character which is accompanied by differences in the form of the sporophylls and sporangia, the structure of the sporangial wall and of the spores, and the whole habit of the strobilus.

Spencerites, and especially *S. insignis,* bears a considerable resemblance to the *Sigillariostrobus Crepini,* of Zeiller, but cannot be united with the genus *Sigillariostrobus,* for the insertion of the sporangia in the latter, as shown in the *Sigillariostrobus ciliatus* of Kidston, is totally different. The author is much indebted both to M. Zeiller and Mr. Kidston, for the loan of their specimens for examination.

The generic and specific characters may provisionally run as follows:—

Spencerites, gen. nov.

Cone consisting of a cylindrical axis, bearing numerous simple sporophylls, arranged spirally, or in crowded alternating verticils.

Sporophylls short, formed of a sub-cylindrical pedicel, expanding into a large peltate lamina.

Sporangia solitary on each sporophyll, inserted, by a narrow base, on the upper surface of the lamina, but free from the pedicel.

Sporangial wall consisting of a single layer of prosenchymatous cells. Spores winged.

1. *Spencerites insignis,* (Will).

Lepidostrobus, sp., Will. "Organization of the Fossil Plants of the Coal-measures," Part 9, 'Phil. Trans.,' 1878, p. 340, figs. 39 to 47 and 52 to 57.

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[*] *Cf.* Solms-Laubach, 'Fossil Botany,' p. 239.

Lepidostrobus insignis, Will. *Loc. cit.* Part 10, 'Phil. Trans.,' 1880, p. 502, figs. 11 and 12.

Lepidodendron Spenceri, Will. *Loc. cit.* Part 16, 'Phil. Trans.,' 1889, p. 199, figs. 19 to 22; Part 19, 'Phil. Trans.,' 1893, p. 24, figs. 41 to 50.

Cone pedunculate; peduncle bractigerous. Whole cone 8—10 mm. in diameter. Axis, 3·5—5 mm. in diameter. Sporophylls, 2—2·5 mm. long; lamina distinctly peltate, vertically elongated.

Sporangia approximately spherical. Spores tetrahedral, becoming spheroidal when free, with a hollow equatorial wing. Maximum diameter of spore, without wing, about 0·14 mm.; with wing, about 0·28 mm. Wood of axis without prominent angles, with or without pith.

Outer cortex containing distinct bands of sclerenchyma.

Locality, near Halifax and Huddersfield.

Horizon, Lower Coal-measures.

2. *Spencerites majusculus*, sp. nov.—Whole cone about 15 mm. in diameter, axis about 9 mm. in diameter. Sporophylls about 3 mm. long; lamina obscurely peltate, as seen in radial section, but greatly elongated tangentially, attaining a breadth of 3 mm.

Sporangia approximately spherical. Spores having the form of quadrants of a sphere, with three narrow wings. Maximum diameter of spore, without wings, about 0·11 mm.; with wings, about 0·15 mm.

Wood of axis with about 30, somewhat prominent, angles; without pith.

Outer cortex uniformly sclerotic.

Locality, near Halifax.

Horizon, Lower Coal-measures.

November 25, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—Lord Lister, F.R.C.S., D.C.L.

Treasurer.—Sir John Evans, K.C.B., D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D., D.C.L., LL.D. Professor Arthur William Rücker, M.A., D.Sc.

Foreign Secretary.—Sir Edward Frankland, K.C.B., D.C.L., LL.D.

Other Members of the Council.—Professor William Grylls Adams, M.A.; Professor Thomas Clifford Allbutt, M.D.; Sir Robert Stawell Ball, M.A.; Rev. Thomas George Bonney, D.Sc.; Professor John Cleland, M.D.; Professor Robert Bellamy Clifton, M.A.; Professor James Alfred Ewing, M.A.; Alfred Bray Kempe, M.A.; John Newport Langley, D.Sc.; Joseph Larmor, D.Sc.; Professor Nevil Story Maskelyne, M.A.; Professor Raphael Meldola, F.C.S.; Professor Edward Bagnall Poulton, M.A.; William James Russell, Ph.D.; Dukinfield Henry Scott, M.A.; Professor Walter Frank Raphael Weldon, M.A.

Professor J. H. van't Hoff, Professor Henri de Lacaze-Duthiers, Professor Wilhelm Pfeffer, and Professor Ferdinand Zirkel were balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "On the Geometrical Treatment of the 'Normal Curve' of Statistics, with especial Reference to Correlation and to the Theory of Error." By W. F. Sheppard, M.A., LL.M., formerly Fellow of Trinity College, Cambridge. Communicated by Professor A. R. Forsyth, F.R.S.

On the Geometrical Treatment of the 'Normal Curve' of Statistics, with especial Reference to Correlation and to the Theory of Error." By W. F. SHEPPARD, M.A., LL.M., formerly Fellow of Trinity College, Cambridge. Communicated by Professor A. R. FORSYTH, F.R.S. Received October 9,—Read November 25, 1897.

(Abstract.)

The object of the paper is, in the first place, to simplify and extend the treatment of normal correlation as expounded by Francis Galton and Karl Pearson; and in the second place to obtain general

formulæ in the theory of error, and to apply them to questions which arise in relation to normal distributions and normal correlation. The method is, throughout, elementary, the use of the differential and integral calculus being avoided, though geometrical infinitesimals are to a certain extent employed.

Geometrical.

The *normal curve* is defined by the property that the product of the abscissa and the subtangent is constant; thus if MP is an ordinate from the base. and PT the tangent, $OM . MT = a^2$, O being a fixed point in the base. The area bounded by the curve and the base OMT is called a *normal figure*. The length $2a$ is the *parameter* of the figure. The definition of the curve leads at once to its projective properties, and also to the formula for the mean value of x^{2n+1} or x^{2n+2}, where x denotes the distance of an element of area from the central ordinate.

If the normal curve or normal figure is rotated about its central ordinate, it generates the normal surface or normal solid. It is proved geometrically that any vertical section of this solid (*i.e.*, any section by a plane perpendicular to its base plane) is a normal figure of the same parameter as the central sections; and, therefore, if the sections of the surface by any series of parallel vertical planes are projected on any plane of the series, the curves so obtained are orthogonal projections of one another with regard to their common base. The converse propositions are also established geometrically.

The volume of the solid is obtained in terms of its central ordinate and of the parameter of vertical sections; and thus it is found that the central ordinate of a normal figure of semi-parameter unity and area unity is $1/\sqrt{2\pi}$.

Let Σ be any closed curve in the base plane. Then it is shown how to construct a curve whose area shall be proportional to the portion of the solid which lies vertically above Σ, *i.e.*, to the volume which would be cut out of the solid by a cylinder having Σ for its cross-section. Thus, when Σ is given, this volume can be measured mechanically.

Statistical.

Let L and M denote the measures of two co-existent attributes, L_1 and M_1 their mean values, a^2 and b^2 the mean squares of the respective deviations from the means, and $ab \cos D$ the mean product of the deviations from the means. Then the angle D is called the *divergence*. The solid of frequency of $(L - L_1)/a \sin D$ and $(M - M_1)/b \sin D$, the planes of reference being inclined at an angle D to one another (so that the included angle is $180° - D$), is called the *correlation-solid*.

It is shown that, whatever the laws of distribution may be, the correlation-solid of the distributions of L and M is the same as that of the distributions of lL$+m$M and l'L$+m'$M, where l, m, l', m' are any constants whatever.

If L and M are distributed "normally," and the distributions are independent, the correlation-solid will be a normal solid. Hence it follows that the distribution of lL$+m$M is also normal.

Galton's definition of normal correlation is adopted; his "coefficient of correlation" being therefore cos D. It is shown that the correlation-solid of two normally correlated distributions is a normal solid, and, therefore, if the distributions of L and of M are normally correlated, the values of lL$+m$M are normally distributed, and the distributions of lL$+m$M and of l'L$+m'$M are normally correlated.

The value of D, in a case of normal correlation, can be obtained without calculating the means, mean squares, and mean product. If we find the medians L_1 and M_1, and form a table of double classification, thus :—

	Below L_1.	Above L_1.
Below M_1.....	P	Q
Above M_1.....	Q	P

then $D = \dfrac{Q}{P+Q} \times 180°$.

If we know the proportions of individuals for which L exceeds values X and X', and the proportions for which M exceeds values Y and Y', we can, for any particular value of D, construct an area representing the proportion of individuals for which L lies between X and X', and M between Y and Y'. The simplest case is that in which the distributions of L and of M are classified in the same way, *e.g.*, according to the "decile" method. The proportions of individuals falling into the 100 classes corresponding to a double decile classification are obtained by constructing a certain figure, which is the same whatever the value of D may be, and moving the figure through a distance equal to D$/360°$ of its whole length. The diagram so obtained contains 100 areas, representing the proportions in the 100 classes in question.

The definitions of independence and of normal correlation are extended to any number of distributions, and it is shown that if the distributions of L, M, N.... are normal, and either independent or correlated, the values of lL $+ m$M $+ n$N $+$.... are normally distributed.

Theory of Error.

Let a community be divided into a number of classes, the proportions in the different classes being $z_1, z_2, z_3 \ldots$, so that $z_1 + z_2 + z_3 + \ldots = 1$. Suppose a random selection of n individuals to be made, the numbers drawn from the different classes being $n(z_1 + \epsilon_1)$, $n(z_2 + \epsilon_2)$, $n(z_3 + \epsilon_3) \ldots$. It is proved geometrically, with the aid of the binomial theorem, that the values of the errors $\epsilon_1, \epsilon_2, \epsilon_3 \ldots$ are normally distributed, and that the distributions are normally correlated. It follows that the values of any expression of the form $\Sigma A \epsilon = A_1 \epsilon_1 + A_2 \epsilon_2 + A_3 \epsilon_3 + \ldots$ are normally distributed. The mean square of $\Sigma A \epsilon$ is shown to be $\{ \Sigma A^2 z - (\Sigma A z)^2 \} \div n$, and the mean product of $\Sigma A \epsilon$ and $\Sigma B \epsilon$ to be $\{ \Sigma A B z - \Sigma A z \cdot \Sigma B z \} \div n$. The applications are of two kinds :—

(1) The values of the probable errors in the determination of certain quantities are obtained, and, in particular, the probable errors in the mean, mean square of deviation, mean product of deviations, and divergence.

(2) Formulæ are obtained for testing particular hypotheses; *e.g.*, whether two distributions (of any kind) are independent; whether a distribution is normal; and whether two normal distributions are correlated.

"Mathematical Contributions to the Theory of Evolution. IV. On the probable Errors of Frequency Constants and on the Influence of Random Selection on Variation and Correlation." By KARL PEARSON, M.A., F.R.S., and L. N. G. FILON, B.A., University College, London. Received October 18,—Read November 25, 1897.

(Abstract.)

1. This memoir starts with a general theorem, by which the probable errors made in calculating the constants of any frequency distribution may be determined. It is shown that these probable errors form a correlated system approximately following the normal law of frequency, whatever be the nature of the original frequency distribution, *i.e.*, whether it be skew or normal. The importance of this result for the theory of evolution is then drawn attention to. It is shown that any selection, whether of size, variation, or correlation, will in general involve a modification not only of the size, but the variation and correlation of the whole complex of correlated organs. The subject of directed selection, of which this random selection is only a special case, is reserved for another memoir, nearly completed.

2. Normal correlation is first dealt with. It is shown that if $\lambda = 0.67449$ and n be the number of observations:

Probable error of a coefficient r_{12} of correlation $= \lambda \dfrac{1-r_{12}^2}{\sqrt{n}}$.

Probable error of r_{12}, for variations with definite values,[*]

$$= \lambda \frac{1-r_{12}^2}{\sqrt{n}} \frac{1}{\sqrt{1+r_{12}^2}}.$$

Correlation of errors in two standard deviations $= r_{12}^2$.

Correlation of errors in a standard deviation and a correlation coefficient $= r_{12}/\sqrt{2}$.

Probable error of a regression coefficient for two organs

$$= \lambda \frac{\sigma_1}{\sigma_2} \frac{\sqrt{1-r_{12}^2}}{\sqrt{n}},$$

where σ_1 and σ_2 are the two standard deviations.

Probable error of a regression coefficient for three organs

$$= \frac{\lambda}{\sqrt{n}} \frac{\sigma_1}{\sigma_2} \frac{\sqrt{1-r_{23}^2-r_{12}^2-r_{13}^2+2r_{12}r_{23}r_{13}}}{1-r_{23}^2}.$$

Correlation between the errors in two correlation coefficients, *i.e.*, r_{12} and r_{13}

$$= r_{23} - \frac{r_{12}r_{13}(1-r_{23}^2-r_{13}^2-r_{12}^2+2r_{12}r_{23}r_{13})}{2(1-r_{13}^2)(1-r_{12}^2)}.$$

Correlation between the errors in two correlation coefficients, *i.e.*, r_{12} and r_{34}

$$= \frac{\left\{ \begin{array}{l} (r_{13}-r_{12}r_{23})(r_{24}-r_{23}r_{34}) + (r_{14}-r_{34}r_{13})(r_{23}-r_{12}r_{13}) \\ + (r_{13}-r_{14}r_{34})(r_{24}-r_{12}r_{14}) + (r_{14}-r_{12}r_{24})(r_{23}-r_{24}r_{34}) \end{array} \right\}}{2(1-r_{12}^2)(1-r_{34}^2)}.$$

3. Skew variation is next dealt with. First the probable errors and correlations of the errors of the constants of the curve

$$y = y_1 \left(1 + \frac{\gamma x}{p+1}\right)^p e^{-\gamma x}$$

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[*] This value for the "array" was erroneously given as that for the absolute value of r_{12} in 'Phil. Trans.,' A, vol. 189, p. 345, but the statement was corrected in 'Roy. Soc. Proc.,' vol. 61, p. 350.

are calculated. The formulæ obtained have been cited and used in a memoir by Karl Pearson and Miss Alice Lee, "On the Distribution of Frequency (Variation and Correlation) of the Barometric Height at divers Stations."[*] We may note the following results :—

Probable error of mean $= \lambda \sigma / \sqrt{n}$.

Probable error of standard deviation σ

$$= \frac{\lambda \sigma}{\sqrt{2n}} \left(1 + \frac{1}{2} \frac{1}{(p+1)^2 S} \right)^{\frac{1}{2}},$$

where $S = \dfrac{B_1}{p} - \dfrac{B_3}{p^3} + \dfrac{B_5}{p^5} - \&c.$, and B_1, B_3, B_5 .... are Bernoulli's numbers.

Probable error of skewness S_k

$$= \lambda \sqrt{\frac{3}{2n}} \frac{1}{\sqrt{1 + 3 (S_k)^2}}, \text{approximately.}$$

Correlation of errors in mean and standard deviation.

$$= - \sqrt{\frac{2}{p+1}} \frac{1}{\sqrt{1 + \frac{1}{2} \dfrac{1}{(p+1)^2 S}}}.$$

Correlation of errors in mean and skewness $= 0$.

Correlation of errors in skewness and standard deviation

$$= \frac{1}{\{1 + 2 (p+1)^2 S\}^{\frac{1}{2}}}.$$

Thus a random selection of size differing in its mean value from the population mean gives in all probability an alteration in variability, but none in skewness. Increased size means decreased variability. A random selection altering variability alters also both size and skewness of distribution.

Probable error of modal frequency y_0

$$= \lambda \frac{y_0}{\sqrt{2n}} \left(1 + \frac{1}{12p} \right)^{\frac{1}{2}}, \text{ approximately,}$$

which is shown to be always less than the probable error of the mean.

The correlation of errors in the moments and their probable errors up to the fourth are also worked out.

4. As a limiting case the probable errors of the skewness, the moments up to the fourth, and of the distance between mean and

[*] About to be published in 'Phil. Trans.'

mode in the case of a random selection from a normal distribution are worked out.

5. Analogous results are next obtained for the skew frequency curves:

$$y = y_1 \left(1 + \frac{x}{a_1}\right)^{m_1} \left(1 - \frac{x}{a_2}\right)^{m_2}$$

and

$$y = y_1 \frac{1}{\{1 + (x/a)^2\}^m} e^{-\nu\tan^{-1}\frac{x}{a}} .$$

It is shown that in these cases the mean size, variation, modal frequency, and skewness are in general all such that their errors are correlated. Hence any selection of size modifies both the variation and skewness of the distribution. This is of considerable importance for the theory of evolution, as in most cases of zoological frequency the distribution is of the second, or tangent curve, type. Hence random selection of size tends to modify not only variation but skewness of distribution. The results are too long to be cited, and their application to special cases involves somewhat lengthy, but not complex arithmetic, which in practical cases we have found much shortened by the use of the " Brunsviga " calculator.

"Further Observations upon the Comparative Physiology of the Suprarenal Capsules." By SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar.[*] Communicated by E. A. SCHÄFER, F.R.S. Received November 2,— Read November 25, 1897.

(From the Physiological Laboratory, University College, London.)

In previous communications[†] I have given experimental evidence in favour of the view that the paired suprarenal bodies and the interrenal gland of Elasmobranch fishes correspond respectively to the medulla and cortex of the suprarenal capsules of the higher Vertebrata. I have further stated, as the result of numerous experiments, that the medullary portion of the suprarenal appears to be absent in Teleosts, the suprarenal bodies in this order of fishes consisting solely of cortex.

Since performing the above series of experiments my attention has been devoted to the general physiological effects of extracts

† 'Physiol. Soc. Proc.,' March 20, 1897; 'Roy. Soc. Proc.,' vol. 61, p. 64, 1897; 'Anat. Anz.,' vol. 13, Nos. 1 and 2, 1897.

obtained from suprarenal capsules.* The extracts were made separately from cortex and medulla, and injected subcutaneously into various mammals. It was noted that the injection of medullary material was invariably fatal if a sufficiently large dose were administered, while the cortical extracts produced no appreciable physiological effects.

In the present communication the above views have been corroborated by testing the effects of the two kinds of gland in Elasmobranchs and of the cortical suprarenals of Teleosts, when extracts of them are injected subcutaneously into small mammals. Naturally only very small quantities of material have been available for this purpose, but the effects upon mice have been quite definite.

The suprarenal bodies obtained from six specimens of *Gadus morrhua* (weighing in a moist state 0·4 gram) were extracted by boiling. The filtered extract was then injected beneath the skin of the back of a mouse. No effects whatever supervened.

Again, the paired bodies from seven specimens of *Scyllium canicula* (weighing when moist 0·3 gram) were similarly extracted, and the filtrate administered to the same mouse (which had remained in perfect health) a few days later. The animal was immediately and powerfully affected. The breathing became very rapid, the limbs became weak, the temperature lowered, and death ensued after convulsions in less than five minutes.

The interrenal gland produced no effects when similarly administered.

[A further experiment with material obtained from *Raja clavata* has been performed. The "axillary hearts" (anterior paired bodies) were removed from three fair-sized specimens, and found to weigh in a moist state 0·2 gram. The interrenal bodies were also removed, and weighed also 0·2 gram.. Extracts were then prepared of each of these, and injected subcutaneously into two separate mice of as nearly as possible the same weight. The mouse which was injected with the extract from the paired suprarenals, was affected in a few minutes. The respirations were very quick at first, afterwards becoming slower and slower. Paralysis quickly came on, first in the hind limbs. All the four limbs were distinctly stiffened before death, which supervened in two hours after injection.

The other mouse, injected with extract of interrenal, died about 24 hours after injection.†—*November* 15.]

These experiments afford further positive evidence of the homology

* ' Physiol. Soc. Proc.,' June 12, 1897; ' Journ. of Physiol.,' vol. 22 (Nos. 1 and 2, Sept. 1), 1897.

.† This result must be attributed to contamination with the paired bodies, and is analogous to the effect one sometimes obtains upon the blood-pressure when interrenal extract is injected intravenously.

of the paired bodies of Elasmobranchs with the medulla of the mammalian suprarenal.   The direct evidence in favour of the homology of the interrenal with the cortex of the suprarenal is mostly morphological and histological, and I have detailed this elsewhere.[*]

"Further Note on the Transplantation and Growth of Mammalian Ova within a Uterine Foster-mother."   By WALTER HEAPE, M.A., Trinity College, Cambridge.   Communicated by Dr. W. H. GASKELL, F.R.S.   Received November 2,— Read November 25, 1897.

In 1890 I recorded[†] an experiment designed to show that it is possible to make use of the uterus of one variety of rabbit as a medium for the growth and complete fœtal development of fertilised ova of another variety of rabbit.

The experiment was further undertaken in order to determine what effect, if any, a uterine foster-mother would have upon her foster-children, and whether or not the presence, during development, of foreign ova in the uterus of a mother would affect offspring of that mother present in the uterus at the same time.   In the experiment above referred to, two fertilised ova were obtained from an Angora doe rabbit which had been inseminated thirty-two hours previously by an Angora buck, and they were inserted into the fallopian tube of a Belgian Hare doe, which had been inseminated three hours before by a buck of the same breed as herself.

In due course the Belgian Hare doe littered six young, four of which were Belgian Hares, while the other two were Angoras.   There was no trace of any cross in any of these young, the four Belgian Hares and the two Angoras were true bred.

The experiment seemed to me to show, so far as a single experiment could show, that a uterine foster-mother has no power of modifying the breed of her foster-children, and that her uterus during gestation and the nourishment she supplies to the embryo is analogous to a bed of soil with its various nutrient constituents.

I had hoped to follow this experiment with others on a larger scale the following year, but was unable to make the attempt until 1893. That year I had extraordinary bad luck with my rabbits.   I used Angoras and Belgian Hares as before, and out of ten Angora does used, four had no ova in their fallopian tubes after being satisfactorily covered, two had dead ova, and only four produced seg-

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[*] 'Zool. Soc. Trans.,' vol. 14, Part III, 1897; 'Birm. Nat. Hist. and Phil. Soc. Proc.,' vol. 10, Part I, 1896; 'Anat. Anz.,' vol. 12, Nos. 9 and 10, 1896.

[†] 'Roy. Soc. Proc.,' vol. 48.

menting ova. But that was not all: I had a stock of fourteen Belgian Hare does, and of the four which were operated on to receive the ova from the four Angora does, three of them died under chloroform and only one bore young, and she had only one young one, and that one was a Belgian Hare.

In 1896 I again attempted the same experiment, using this time Dutch and Belgian Hare rabbits; and again I failed, but from a different cause. The Dutch rabbits produced segmenting ova, and the Belgian Hare does stood the operation perfectly satisfactorily, but they were bad breeders. Two of them had only one young one each, one had two, and one six young ones; they were all undoubtedly Belgian Hares.

These Belgian Hare does I had kept for one, some of them for two years, without allowing them to breed, and I am inclined to think that was the reason why they were not so prolific as usual. I considered also that their disinclination to breed might operate adversely on the foreign ova which were introduced, and so check their development.

This year I made five experiments, using again Dutch and Belgian Hare rabbits. The method adopted was the same as that already described. A Dutch doe was covered by a Dutch buck, twenty-four or thirty hours later a Belgian Hare doe was covered by a Belgian Hare buck; the Dutch doe was then killed, and segmenting ova, by this time divided into two or four segments, were taken from her fallopian tube and placed into the open anterior end of the fallopian tube of the Belgian Hare doe.

The operation is a very simple one. The Belgian Hare doe is put under anæsthetics and stretched out on her stomach. A longitudinal incision, 2 in. long, is then made through the skin at a place $1\frac{1}{2}$ to $3\frac{1}{2}$ in. from the anterior edge of the pelvis, and on a level with the ventral border of the lumbar muscles. A smaller incision is then made through the body-wall just ventral to the lumbar muscles, and the anterior end of the fallopian tube is readily found and pulled out through the opening with the help of a pair of forceps. The foreign ova are then taken out of their maternal fallopian tube on the point of a spear-headed needle, the foster-mother's infundibulum is held open with a pair of forceps and the ova placed well within the anterior end of her fallopian tube; after pushing the latter gently back again and washing with some antiseptic solution, the wound is sewn up and dressed with collodion and cotton-wool.

In one case the rabbit died under anæsthetics before the operation began, from heart failure (degeneration), but in the other four cases the recovery was rapid and no discomfort even shown, after the effects of the anæsthetic had worn off.

Of these four experiments, in one case the Belgian Hare doe proved barren; in another she gave birth to eight Belgian Hare

young; in a third she gave birth to eleven Belgian Hare young; while in the fourth case the Belgian Hare doe gave birth to seven young, of which five were Belgian Hares and two were apparently Dutch.

When the young began to run about, I observed that both these Dutch young were irregularly marked, and at first I was inclined to think it was possible, after all, either—

(1) That the Belgian Hare foster-mother had influenced the Dutch fertilised ova; or
(2) That these two young were really a cross between Dutch and Belgian Hare.

In order to test the first of these possibilities, I made the following experiment. I put the same Dutch buck which had been used in the foregoing experiment, to a thorough-bred Dutch doe, and she produced a litter every one of which was badly marked, most if not all of them, were as badly marked as the Dutch foster-children, while certain of them were even worse marked.

This Dutch doe I bred myself; she was one of an exceptionally good litter, and I obtained out of her by another buck, the previous year, a very good litter. I have no doubt the bad marking of the young in this last litter is the fault of the buck now being used; he is not well bred. Dutch rabbits frequently have some badly marked young in their litters, even when they are themselves excellently well-bred animals, but the litter described above consists altogether of outrageously badly marked young; in fact most of them could not be recognised as Dutch at all, as far as their marking is concerned.

This experiment therefore shows that the bad marking of the foster-children can be fully accounted for by the fact that their father is badly bred, and it is not necessary therefore to suppose that the foster-mother is the cause of the irregularity.

The second possibility is, however, far more difficult to test, and I do not hold that, under the circumstances attending my experiment, it is possible to determine it quite satisfactorily; at the same time I think a strong case of probability can be made out.

With regard to the possibility of getting a cross between the Dutch buck and the Belgian Hare foster-mother, in consequence of my experiment, it is by no means impossible this should have been done; for it must be remembered that when the foreign Dutch segmenting ova were introduced into the fallopian tube of the Belgian Hare foster-mother, they were still surrounded by spermatozoa from the Dutch buck, spermatozoa that were still alive though perhaps failing in vigour; and numbers of these Dutch spermatozoa, there can be little doubt, were introduced into the foster-mother's fallopian tube along with the Dutch fertilised ova.

Then again the Belgian Hare foster-mother had not ovulated when the operation was performed ; ovulation in the rabbit does not take place until about ten hours have elapsed after the act of coition, and the operation was performed one hour or less after coition.

It was quite possible then, that when ovulation did take place, some nine hours later, some Dutch spermatozoa might still be alive and in a condition to fertilise the Belgian Hare ova when they were produced.

But the Belgian Hare doe had been inseminated by a Belgian Hare buck just before the operation ; and the spermatozoa from this buck would arrive at the end of the fallopian tube before ovulation took place ; this spermatozoa would be at least twenty-four hours younger than the foreign Dutch spermatozoa, and both more vigorous and in far greater numbers than the foreign spermatozoa.

It seems to me that the possibilities are distinctly in favour of the host of younger and more vigorous Belgian Hare spermatozoa beating the comparatively small body of older and less vigorous foreign Dutch spermatozoa, in the struggle for the Belgian Hare ova, but at the same time it is possible the latter won, and that, in the case now under consideration, these two badly marked young are the result of a cross between Dutch spermatozoa and Belgian Hare ova.

The only way to test this at all seemed to be by crossing the same Dutch buck with Belgian Hare does, and comparing the offspring of such crosses with the young foster-children. But even this could not be conclusive proof no matter what result was obtained, for, as is well known to rabbit breeders, although probably the majority of offspring got by crossing two distinct breeds will be of a nondescript character, yet cases continually occur where apparently thorough-bred young, of one breed or the other or perhaps of both breeds, are produced in the same litter together with obvious cross-breds.

However, I crossed this Dutch buck with two Belgian Hare does and in the first case there were five young, one of which was Belgian Hare purely in colour, two were coloured like a Belgian Hare with white spots or patches here and there, one was black with a couple of white spots, and one was white speckled with the characteristic Belgian Hare brown colour.

In the litter out of the second Belgian Hare doe by this same Dutch buck there were also five young, two of which were of pure Belgian Hare colour, one was a light fawn colour mixed with a bluish dun and with one white shoulder and fore leg, a fourth was a slightly darker shade of the same colour with white fore feet, and the fifth a very light fawn underneath, while somewhat darker on the back and with white fore feet and white dash on the tail.

None of the ten young produced in these two cross-bred litters at

all closely resembled their Dutch father, but three of them were
apparently thorough-bred Belgian Hares; it certainly seems that in
the case of these animals the Belgian Hare strain is much the
stronger of the two; at the same time the father's influence is seen
in the very general introduction of white and in the fawn and dun
colours of certain of the young.

With regard to the foster-children, one of them unfortunately died
at an early age, but the second one lived and is now more typically
Dutch than it was when very young.   It is coloured like its mother,
fawn and white, and has no trace of the bluish dun shade noticeable
here and there in its father's coat.   Its head, feet, and legs, are
remarkably like its mother's, the saddle on the back is fairly well
defined, especially on one side, and the main faults are white tips
to the ears and patches of white across the fawn colour of the back
and sides.

In reviewing the whole question one may claim :—

(1) That in two litters got by the Dutch buck out of Belgian
    Hare does, there were ten young ones, not one of which so
    nearly approaches the Dutch type as does the single Dutch
    young one borne by the Belgian Hare foster-mother.

(2) That the bad marking of the young got by the Dutch buck
    out of a pure-bred Dutch doe is shown to be the fault of the
    father, and that, consequently, it is not surprising his other
    offspring, the foster-child, should also be badly marked.

(3) That the probability of the Dutch buck begetting character-
    istic Dutch young when crossed with a doe of another
    species, is reduced to a minimum; while, on the other hand,
    the probability is increased that a young one, with such
    strongly marked Dutch characteristics as the foster-child is
    possessed of, is derived from the ovum of a Dutch mother.

(4) That the chance of producing a cross-bred young one out of
    the Belgian Hare foster-mother by the help of Dutch
    spermatozoa, which was twenty-four or more hours old
    when introduced into her fallopian tube, is remote; and is
    rendered still more improbable when it is remembered that
    fresh Belgian Hare spermatozoa in ample quantity would
    also be present.

(5) That the similarity of the result now obtained with that
    obtained in 1890, is striking evidence in favour of my con-
    tention that these experiments present strong evidence
    towards the proof, (1) that it is possible to make use of the
    uterus of one variety of rabbit as a medium for the growth
    and complete foetal development of fertilised ova of another
    variety of rabbit; and (2) that the uterine foster-mother

exerts no modifying influence upon her foster-children in so far as can be tested by the examination of a single generation.

(6) It follows, if the above is true, that in case telegony be actually demonstrated, the characteristics of a primary husband which are transmitted to the offspring got by a secondary husband, can only be so transmitted through the ovarian ova of the mother.

Romanes, in his work on 'Darwin and after Darwin,' vol. 2, pp. 146—148, refers to my earliest experiment; he thereon remarks that rabbits when crossed in the ordinary way never throw intermediate characters, and that the experiment is clearly without significance as far as it bears upon the inheritance of acquired characters.

Mr. Romanes does not give his authority for the statement that rabbits when crossed never throw intermediate characters, and I venture to think he was mistaken in his view; that they do produce young which are apparently pure bred of the one type or the other, and possibly both, in the same litter is no doubt true, but it is also true that some at any rate of the young got by crossing are of an intermediate character. An examination of the litters I got by crossing a Dutch buck with Belgian Hare does confirms this view.

"Antagonistic Muscles and Reciprocal Innervation. Fourth Note." By C. S. SHERRINGTON, M.A., M.D., F.R.S., University College, Liverpool, and E. H. HERING, M.D. (Prague). Received August 4,—Read November 18, 1897.

(From the Physiological Laboratory, University College, Liverpool.)

The object of the present communication is to report to the Society further on the occurrence in so-called " voluntary " muscles of *inhibition* as well as of *contraction*, as result of excitation of the *cortex cerebri*. We have obtained by excitation of the cerebral cortex some remarkable instances of what one of us has described[*] under the name of "*reciprocal innervation*," that is, a species of co-ordinate innervation in which the relaxation of one set of a co-ordinated complexus of muscle-groups occurs as accompaniment of the active contraction of another set.

The experiments, the subject of the present communication, have been carried out in the monkey (*Macacus cynocephalus*) and the cat. The appropriate region of the cortex cerebri has been freely exposed after removal of part of the cranium and subsequent

[*] 'Roy. Soc. Proc.,' vol. 60.

slitting and turning aside of the dura mater. The cortex has then been stimulated by rapidly repeated induction shocks, obtained by the Du Bois inductorium. The Helmholtz equaliser has always been employed. The intensity of the faradic currents used has been usually such as to be barely perceptible when the platinum electrodes were applied to the tongue-tip. The anæsthetics used have been either ether alone or ether mixed with chloroform in equal volumes. The degree of narcotisation forms an important condition for the prosecution of the observations. When the narcosis is too profound the results to be recorded are much less obvious than when the narcosis is less deep. It is best, starting with the animal in a condition of deep etherisation, to allow that condition gradually to diminish. As this is done it almost constantly happens that at a certain stage of anæsthesia the limbs instead of hanging slack and flaccid assume and maintain a position of flexion at certain joints, notably at elbow and hip. This condition of tonic contraction having been assumed, the narcosis is, as far as possible, kept at that particular grade of intensity. The area of *cortex cerebri* previously ascertained to produce under faradisation extension of the elbow-joint or hip-joint is then excited.

For clearness of description we will suppose that the left hemisphere is excited, and that, therefore, the limb affected is the right. The result of excitation of the appropriate focus in the cortex, *e.g.*, that presiding over extension of the elbow, is an immediate relaxation of the biceps, with active contraction of the triceps. As regards the condition of the biceps, the relaxation is usually so striking that merely to place the finger on it is enough to convince the observer that the muscle relaxes. The following is, however, a good mode of studying the phenomenon : In a monkey with strongly developed musculature the forearm, maintained by the above-mentioned steady tonic flexion at an angle of somewhat less than 90° with the upper arm, is lightly supported by the one hand of the observer, while with the finger and thumb of the other the belly of the contracted biceps is felt through the thin skin of the upper arm. On exciting the cortex the contracted mass becomes suddenly soft—as it were melting under the observer's touch. At the same time the observer's hand supporting the animal's forearm tends to be pushed down with a force unmistakably greater than that which the mere weight of the limb would exert. If the triceps itself be felt at this time it is easy to perceive that it enters contraction, becoming increasingly hard and tense, even when its points of attachment are allowed to approximate, and the passive tensile strain in it should diminish. If the limb be left unsupported the movement is one of simple extension at the elbow-joint. On discontinuing the excitation of the cortex the forearm usually immediately, or almost immediately, returns to its

previous posture of flexion, which is again as before steadily maintained.

Conversely, when, as not unfrequently occurs in conditions of narcosis resembling that above referred to, the arm has assumed a posture of extension, and this is tonic and maintained, the opportunity is taken to excite the appropriate focus in the cortex, previously ascertained, for flexion of forearm or upper arm. Triceps is then found to relax, and the biceps at the same time enters into active contraction. If the biceps be hindered from actually moving the arm, the prominence at the back of the upper arm due to the contracted triceps is seen simply to sink down and become flattened. When examined by palpation the muscle is felt to become more or less suddenly soft, and the biceps at the same time to become tenser than before. The movement of the limb, when allowed to proceed unhindered, is flexion with some supination. It is noteworthy that in this experiment not every part of the large triceps mass becomes relaxed; a part of the muscle which extends from humerus to the scapula does not in this experiment relax with the rest of the muscle. This part, if the scapula be fixed, acts as a retractor of the upper arm, and is not necessarily an antagonist of the flexors of the elbow. This part of the triceps we observed sometimes enter active contraction at the same time as the flexors of the elbow. It should be remarked that under use of currents of moderate intensity we find that not from one and the same spot in the cortex can relaxation and contraction of a given muscle be evoked at different times, but that the two effects are to be found at different, sometimes widely separate, points of the cortex, and are there found regularly.

We have obtained analogous results in the muscles acting at the hip-joint. When in the narcotised animal the hip-joint is being maintained in flexion, the thighs being drawn up on the trunk, excitation of the region of the cortex previously ascertained when the limbs hang slack to evoke extension of the hip, produces relaxation of the flexors of the hip and at the same time active contraction of the extensors of the thigh. We examined particularly the psoas-iliacus, and the tensor fasciæ femoris, also the short and long adductor muscles. Each of these was found to relax under appro·priate cortical excitation. If the knee were held by the observer it was found at the time of relaxation of the flexors of the hip to be forced downward by active extension of the hip.

Similarly with other groups of antagonistic muscles both those of the small apical joints of the limb, *e.g.*, flexors and extensors of the digits, and those of the large proximal joints, *e.g.*, adductors and abductors of the shoulder. At these also instances of reciprocal innervation were obtained.

That a part of the *triceps brachii* (that retracting the upper arm)

should actively contract exactly when another part (that extending the elbow) becomes relaxed is exactly comparable with a phenomenon which has been described by one of us in treating of spinal reflexes, both in the triceps itself and in the quadriceps femoris. And we have similarly seen in the quadriceps femoris, on exciting the cortical region yielding extension of the hip, a relaxation of a part of the quadriceps (a part which flexes the hip) with contraction of another part (which extends the knee).

We hope, in a longer communication, in which the literature can be dealt with, to give a more detailed account of other instances of co-ordination, in which inhibition of the contraction of the so-called voluntary muscles makes its appearance in result of excitation of the cortex cerebri. The examples cited in the present Note are evidently intimately related to those to which attention has been already called in Second* and Third Notes† on the co-ordination of antagonistic muscles already presented to the Society.

Addendum.

Since the above was written, one of us (C. S. S.) has had opportunity to perform further experiments both on the monkey and the cat, and has carried out excitation of the fibres of the internal capsule, as well as excitation of the cortex cerebri. Knowledge is at present completely vague as to which of the many elements in the cortex constitutes the locus of genesis of the reaction obtained by electrical stimuli applied from the cerebral surface. It is therefore conceivable that the elements of the pyramidal tract are only mediately excited by moderate faradisation of the cortex. If so, inhibitory effects produced by excitation of the cortex might likely enough *not* occur under direct excitation of the cut fibres of the capsula interna.

As a matter of fact, however, the results obtained from the internal capsule have been as striking as those obtained from the cortex itself. From separate points of the cross-section of the capsula, relaxation of various muscles has been evoked.

Among the muscles, inhibition of which has been directly observed, are supinator longus, and biceps brachii, the triceps, the deltoid, the extensor cruris, the hamstring group, the flexor muscles of the ankle joint, and the sternomastoid.

The spots in the cross-section of the capsula which have yielded the inhibitions are constant, that is, the position of each when observed has remained constant throughout the experiment. The area of the capsular cross-section at which the inhibition of the

* ' Roy. Soc. Proc.,' vol. 52.
† ' Roy. Soc. Proc.,' vol. 60.

activity of, *e.g.*, the triceps, muscle can be evoked is separate from (that is to say not the same as) that area whence excitation evokes contraction of the triceps (or of that part of the triceps, inhibition of which is now referred to). On the other hand, the area of the section of the internal capsule, whence inhibition of the muscle is elicited, corresponds with the area whence contraction of its antagonistic muscles can be evoked. Yet synchronous contraction of such pairs of muscles as gastrocnemius and peroneus longus is obtainable from the cortex.

The observations make it clear that "*reciprocal innervation*" in antagonistic muscles is obtainable by excitation of the fibres of the internal capsule. It is probable, therefore, that the inhibition elicitable from the cortex cerebri is not due to an interaction of cortical neurons one with another. The variety of nervous reaction in which I have been able to establish existence of the reciprocal form of muscular co-ordination is now pretty extensive. In some the condition described in the previous (3rd) Note (the state shewn to ensue upon removal of the cerebrum, and in that Note spoken of as " decerebrate rigidity ") was conducive to the result; in others the cerebrum was of course not removed. The reactions examined for the phenomenon with positive result include those initiated by excitation of

(1) the skin and skin nerves (with " decerebrate rigidity "),
(2) the muscles and afferent nerves of muscle (with " decerebrate rigidity "),
(3) the dorsal (posterior) columns of the cord (with " decerebrate rigidity "),
(4) of the cerebellum (with " decerebrate rigidity "),
(5) of the crusta cerebri (with " decerebrate rigidity "),
(6) of the internal capsule,
(7) of the optic radiations,
(8) of the Rolandic cortex,
(9) of the occipital (visual) cortex.

C. S. S., *November* 3, 1897.

"On certain Media for the Cultivation of the Bacillus of Tubercle."* By ARTHUR RANSOME, M.D., F.R.S. Received November 13,—Read November 25, 1897.

In May, 1894, a communication was made to the Society by Professor Delepine and myself, " On the Influence of certain Natural Agents on the Virulence of the Tubercle-Bacillus."

The conclusions drawn from the experiments recorded in this paper were:—

(1) That finely divided tuberculous matter, such as pure cultures of the bacillus, or tuberculous matter derived from sputum, in daylight and in free currents of air is rapidly deprived of virulence;

(2) That even in the dark, although the action is retarded, fresh air has still some disinfecting influence; and

(3) That in the absence of air, or in confined air, the bacillus retains its power for long periods of time.

These observations afforded an explanation of the immunity of certain places, and the danger of infection in others. They show that where tuberculous sputum is exposed to sufficient light and air, to deprive it of virulence before it can be dried up and powdered into dust, no danger of infection need be dreaded. It would appear further, from this research and others, that it is only when there is sufficient organic material in the air, derived from impure ground air, or from the reek of human bodies, that the tubercle bacillus can retain its existence and its virulent power. Long-lived though it may be under these latter conditions, it is rapidly disinfected by the natural agencies of fresh air and sunlight; so rapidly that, when these agents are present, even in comparatively moderate degree, the tuberculous material cannot reach its dangerous state of dust before it is deprived of all power of doing harm.

But, in addition to the above-mentioned researches, it seemed desirable that an attempt should be made to ascertain what part was played respectively by the several forms of organic impurity that are present in insanitary dwellings. Hitherto, so far as I know, no attempt has been made, in the laboratory or elsewhere, to imitate the actual conditions that prevail in such houses. It was determined, therefore, to collect the aqueous vapours arising from the ground or from human bodies, and to submit these products to the test of trying whether they would serve as cultivating media for the bacillus of tubercle.

Many years ago in a research, the particulars of which are given in an appendix to my treatise on " Stethometry," I examined the condensed aqueous vapour of the breath, in health and disease, and ascertained the quantity of organic matter that it contained. The breath of fifteen healthy persons and of twenty-seven cases of disease was examined chemically by Wanklyn's method of water analysis, and microscopically. The fact of chief importance obtained was, that every specimen contained a small, but appreciable, quantity of both free and organic ammonia. The quantity from the cases of disease varied considerably, but that from healthy persons was remarkably constant, varying from 0·325 milligram to 0·45 per 100 minims of

the fluid collected, the average being 0·4. Hence, by calculation, we obtain the rough estimate that about 3 grs. of organic matter is given off from a man's lungs in the course of twenty-four hours. Doubtless a very small amount, but sufficient to render the aqueous vapour thus thrown off more impure than most sewage water, and ample in quantity, to foster the growth of organic germs.

It was the result of this research that induced me to try to cultivate the bacillus of tubercle upon these and similar organic fluids, such as were likely to be met with in dwelling-houses.

By means of a simple freezing mixture of ice and salt it was easy to condense the aqueous vapour, both of the breath and that coming from ground air; and, in order to make the inquiry more complete, the vapour of the breath was collected in a flask, surrounded by this mixture, from both healthy and diseased sources. In other words, both healthy persons and those affected by phthisis were prevailed upon to breathe into the flask, until a sufficient quantity of aqueous fluid had been obtained.

With another apparatus, consisting of a framework supporting beakers containing freezing mixture, collections of aqueous fluid were obtained from "ground air" coming from a wine cellar in a gravelly subsoil, and from cellars under several low-lying, unsanitary cottages in Southampton. Some of the moisture from a weaving-shed in Blackburn was also thus collected and used as a cultivating medium. The composition of these latter fluids is given below:—

Table I.—Composition of condensed Aqueous Vapours from following sources.

Sources of fluids.	Parts by weight of ammonias per 100,000.		Grains per gallon of ammonias.	
	Free and saline.	Albuminoid.	Free and saline.	Albuminoid.
Healthy breath	1·022	3·568	1·135	2·497
Phthisical breath	0·973	2·598	0·681	1·816
Bournemouth cellar air	0·649	1·622	0·454	1·135
Southampton cellar air......	2·141	3·893	1·498	2·724
Pure sandy soil	0·020	0·030		
Blackburn weaving sheds (average) (humidified)	0·319	0·082	0·223	0·057
Thames sewage at South Outfall (Keats)	2·309	3·893	1·498	2·724

These several liquids were carefully sterilised by repeated boilings,

and were then used, in various ways, for the cultivation of the bacillus of tubercle.*

Two well-grown specimens of pure cultivations were obtained (both through Dr. Childs), one (A) from the Institute of Preventive Medicine, the other (B) from a private source, but the latter specimen could not be guaranted as human bacillus, it was therefore labelled as of doubtful origin, and the cultivations made with it were kept separate.

In order to test the activity of these cultures they were each, in the first instance, sown upon (*a*) sterilised blood-serum, and (*b*) upon " glycerine agar peptone," as these media were known to be the best for cultivating purposes, and the results could then with advantage be compared with those from the other materials used.

Both specimens were found to be capable of active growth, though the cultivation (A) was somewhat tardy.

Table II.

Media.		Date of. inoculation.	Periods of incubation (at 35° C.).			
			2 weeks.	4 weeks.	8 weeks.	12 weeks and upwards.
Blood serum.....	A	April 3	..	×	× ×	× × ×
Agar peptone	A	„ „	..	×	× ×	× × ×
Blood serum.....	B	„ „	× ×	× × ×	× × ×	
Glycerine agar ...	B	„ 13	×	× ×	× × ×	× × ×
Agar peptone	B	„ 3	× ×	× × ×	× × ×	

The crosses denote degrees of growth. One × means the first appearance of a colony. Two × ×, two or more colonies, evidently growing. Three × × ×, growth extending over medium.

It was then thought well, in the first instance, to attempt to cultivate the bacillus upon media, on which it grows with difficulty, without the presence of added peptones; in other words, to find out whether the presence of the condensed organic fluids from the sources that have been mentioned would replace the peptones.

Accordingly, simple agar jelly, with 6 per cent. of glycerine, was made with each of the fluids mentioned, after careful sterilisation. Tubes were charged with these several compounds, inoculated with looped platinum wire, lightly charged, stoppered with sterilised

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* The various manipulations required in this inquiry were carried out chiefly by Mr. Tanner, in his Bacteriological Laboratory, at Bournemouth, and to his carefulness and skill much of the success attained is due.

wool, capped, and placed in an incubator, kept at a temperature of 35° C. At the same time, slips of potato, after thorough sterilisation, were soaked in the fluids and inoculated and similarly disposed of.

As a control experiment, the agar jelly was made with simple distilled water and glycerine, charged and disposed of in the same way.

The results of these several experiments are shown on the two following tables. It will be observed that, out of the eighteen specimens, only two (two of those from the impure cellars) failed to produce growth to some extent; those that did best were the fluids from the cellar in porous soil, and those condensed from the breath of phthisical patients. But all kinds of organic fluid showed growth on either agar jelly or potato.

Table III.

No.		Date of inocula-tion.	Periods of incubation and growth (in incubator at 35° C.).			
			2 weeks.	4 weeks.	8 weeks.	12 weeks and upwards.
	Media :—Agar c̄ 5 per cent. glycerine Condensed vapour from the following sources :—	April 13	..	×	× ×	× ×
1	Cellar in pure porous soil...............	April 3	×	× ×	× × ×	× × ×
2	Ditto	„ „	×	× ×	× × ×	× × × ×
3	Ditto	„ 10	..	..	×	faint
4	Ditto	„ „	..	..	×	„
5	Impure cellar on clay	„ „	..	..	..	blank
6	Ditto	„ „	..	..	..	„
7	Healthy breath.....	„ „	..	×	×	× ×
8	Ditto	„ „	..	×	×	× ×
9	Phthisical breath ...	„ 3	×	× ×	× × ×	× × × } blackened
10	Ditto	„ „	×	× ×	× × ×	× × × }

There is thus some evidence that the organic fluids facilitated cultivation to some extent; experienced bacteriologists, who have attempted to use simple potato or glycerine agar as the cultivating medium, have assured me that failure is much more common than success, and that the growth, when it does take place, is usually very slow. With the organic fluids there were only two failures, and growth was fairly rapid.

Table IV.

No.		Date of inocula-tion.	Periods of incubation and growth (in incubator at 35° C.).			
			2 weeks.	4 weeks.	8 weeks.	12 weeks and upwards.
	Media :—Sterilised potato, and the vapours as above					
1	From cellar in pure porous soil	April 3	x	x x x	x x x x	x x x x
2	Ditto	,, ,,	x	x x	x x	Feeble
3	Impure cellar in clay	,, 10	..	..	x x	x x x
4	Ditto	,, ,,	..	..	x x	x x x
5	Healthy breath	,, ,,	..	x	x x	x x
6	Ditto	,, ,,	..	x x	x x	x x x
7	Phthisical breath	,, 3	..	x	x x	x x x

In the next series of trials, it was decided to use as the material bases some non-nitrogenous substance, and attempts were made to employ pieces of wood, cork, cotton-wool, and fine spun glass, the last named at the suggestion of a distinguished bacteriologist. None of these bases were found to be satisfactory; and at length it was determined to use a particularly pure "filter-paper," manufactured by Messrs. Schleicher and Schüll, from which even the salts had been extracted by washing with hydrochloric and hydrofluoric acids.* This paper was folded in a convenient form, sterilised, inserted in the test-tubes, and charged with the several organic fluids, to which, as before, 6 per cent. of pure glycerine had been added. It was then inoculated, stoppered as before, and in the first trials these tubes were placed in the incubator at the usual temperature of 35° C.

The results are shown on Table V.

It will be seen that some degree of success was attained in twelve out of fifteen specimens of the organic fluids. The degree of growth was also much the same as in the previous series, though perhaps slightly less vigorous.

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* Each of these filter-papers, analysed for me by the Kjeldahl process, by Sir H. Roscoe's assistant, was found to contain only 0·1 milligram of nitrogen.

Table V.

No.	Media:—Chemically pure filter-paper and condensed fluids с 6 per cent. glycerine.	Date of inoculation.	2 weeks.	4 weeks.	8 weeks.	12 weeks.	Remarks.
	Culture A.						
1	From pure cellar air...........	May 23	:	× ×	× ×	× × ×	
2	,, ,, ...	,,	:	× ×	×	× × ×	
3	,, impure cellar air......	,,	× ×		× ×	× × ×	
4	,, ,, ,, ...	,,	×	×	×	×	
5	,, weaver's shed	June 5	:	× ×/×	× ×	× × ×	
6	,, ,, ,, ...	,,	:	× ×	× ×	× × ×	
7	,, healthy breath......	,,	:	× ×	× ×	× × ×	
8	,, ,, ,, ...	,,	:	×	×	× × ×	
9	,, phthisical breath.....	May 23	×	× ×	×	× × ×	
10	,, ,, ,, ...	,,			..	.. }	Medium found to contain free ammonia.
11	Distilled water	,,	:	?	..	..	
12	,, ,, ...	,,	:	?	:	:	
	Culture B.						
1	Pure cellar air........	,,	?	×	× ×	× ×	
2	Impure ditto.........	,,	P.				
3	Weaver's shed........	June 5	P.	× ×	× ×	× × ×	
4	Healthy breath.......	,,				×, × ×	
5	Phthisical breath.....	May 23					
6	Distilled water.......	,,					

Periods of cultivation (in incubator at 35° C.).

It was now determined to try to do without the help of the glycerine, which, as is well known, so greatly assists the ordinary cultivations of the bacillus. Accordingly, four tubes with simple filter-paper as the supporting medium, and condensed fluids, from the breath of a healthy person, and from that of a phthisical patient, as nutrient fluids, were inoculated, and no glycerine was added. In these tubes the same cultivation was used as in the previous experiments.

Shortly afterwards, two similar tubes with fluid from healthy breath alone, but with 5 per cent. of glycerine, were sown with the same cultivation, and were left at the ordinary temperature of the laboratory, about 21° C. (see Table VI).

All of the former group took on active growth within four weeks, and one of the latter. In other words, it was proved that pure filter-paper, moistened with these condensed fluids, alone would suffice to nourish and promote the growth of the bacillus, and, further, that this growth would take place at ordinary temperatures. It may hence be concluded that when this organic fluid is present in ordinary dwellings, the bacillus may grow at the temperature of living rooms as well as at the temperature of 35° C.

In September, 1896, another attempt to test this point was made by inoculating a dozen more tubes in which the various condensed fluids were employed as nutrients. Some of them were placed in the incubator, the others being placed outside.

In this series, however, a sub-culture on agar peptone, taken from the old Preventive Institute tube, was used as the seed; and it was soon evident that this sub-culture had greatly declined in vigour. For three months no perceptible growth took place on any of the specimens, and then only on phthisical breath to a very slight extent. Although they must be counted for the most part as failures, the results of the inoculations are given in Table VI.

In consequence of this failure in vigour of the last used cultivation, a fresh series of eight tubes was commenced on October 31 with the same cultivation, which also failed.

Then, in February, 1897, through the kindness of Dr. Childs and of Dr. Curtis, a fresh tube of apparently vigorous cultivation of the tubercle bacillus, guaranteed to be of human origin, was obtained from University College, London.

By way of control, this culture was sown upon blood serum and upon agar peptone, and incubated at 37° C., and a copious growth was found to be commencing on the blood serum within ten days time (see Table IX).

Two sets of tubes were then prepared of condensed vapour from breath, and from ground air from a pure sandy soil. No glycerine was added; but for the solid medium, in some instances, the pure

Table VI.

No.		Date of inoculation.	Periods of cultivation.				
			2 weeks.	4 weeks.	8 weeks.	12 weeks.	16 weeks
	Media :—Pure filter-paper with condensed fluids alone (no glycerine).						
	Culture A.						
	In incubator at 35° C.						
1	Healthy breath	July 21	..	×	× ×	× ×	
2	„ „	„	..	×	×	×	
3	Phthisical breath......	„	..	×	×	×	
4	„ „	„	×	× ×	× × ×	× × ×	
	Ditto with 5 per cent. glycerine at temperature of laboratory (or about 21° C.).						
1	Healthy breath	„					
2	„ „	„	×	× ×	× ×	× ×	
	Sub-culture from A.		1 mnth.	2 mnths.	3 mnths.	4 mnths.	
	Same medium, without glycerine.						
1	Phthisical breath	Sept. 17	..	..	..	×	×
2	Ditto, 35° C.	„ „					
3	Ditto, ordinary temp...	„ „					
4	Ditto „ „ ..	„ „					
5	Healthy br·ath	„ „					
6	Ditto, 35° C..........	„ „					
7	Ditto, ordinary temp...	„ „					
8	Ditto „ „ ..	„ „					
9	Blackburn shed, 35° C.	„ 24					
10	Ditto „	„ „					
11	Ditto, ordinary temp...	„ „					
12	Ditto „ ..	„ „					

filter-paper was employed; in others, an ordinary lining paper, containing a little size, but carefully sterilised, was used.

Some of these were placed in the incubator at a temperature of 37° C., as this higher degree was thought more favourable to growth; others were left in the dark at the ordinary temperature of the laboratory. The results are shown on the following Tables VII and VIII.

Table VII.

No.	Media	Temperature	Date of inoculation	Periods of cultivation				Remarks
				2 weeks.	4 weeks.	2 months.	3 months.	
	Media:—Pure filter-paper, $\bar{c}$ following vapours and ¼ per cent. gelatine.							
96	Pure ground air	37° C.	Feb. 10	x	x	x x	x x	
97	,, ,,	,,	,,	x	x	x x	x x	
100	Healthy breath	,,	,,	x	x	x	x	
101	,, ,,	,,	,,				x	
	Same, without gelatine.							
104	Pure ground air	22	,,	x	x	x x	x x	Removed from incubator 9th day.
105	,,	37	,,	x x	x x	x x	x x	Ditto.
106	,,	,,	Mar. 2	x x	x x	x x	x x	Ditto.
122	,,	22	,,	:	x	x	x	
123	,,	37	,,	x x	x x	x x	sent away	
124	,,	,,	,,	x x	x x	x x	x x	
125	,,	,,	,,	x x	x x	x x	x x	
126	,,	,,	,,	x x	x x	x x	x x	
127	,,	,,	,,	x x	x x	x x	x x	
128	,,	,,	22	x x	x x	x x	x x	
116	Healthy breath	37	,,	x	x	x	x x	
117	,, ,,	,,	,,					

Table VIII.

No.	Media	Temperature	Date of inoculation	2 weeks	4 weeks	2 months	3 months	Remarks
	Media:—Lining wall-paper, δ vapours and ½ per cent. gelatine.							
98	Pure ground air	37° C.	Feb. 10	×	× × ×	× × ×	× ×	
99	Ditto	,,	,,	×	× × ×	× × ×	× × ×	Removed from incubator 9th day
102	Healthy breath	,,	,,	× ×	× ×	× ×	× ×	
103	Ditto	,,	,,	× ×	× ×	× ×	× ×	
	Same, without gelatine.							
108	Pure ground air	22	,,	×	× ×	× ×	× ×	
107	Ditto	37	,,	× ×	× × ×	× × ×	× × ×	
109	Ditto	,,	,,	× ×	× ×	× ×	× ×	
110	Ditto	,,	,,	× ×	× ×	× × ×	× × ×	
111	Ditto	,,	,,	× ×	× ×	× ×	× ×	Ditto.
112	Ditto	22	Mar. 2	×	× ×	× ×	× ×	
129	Ditto	,,	,,	×	× ×	× ×	sent away	
130	Ditto	,,	,,	× ×	× ×	× ×	× ×	
134	Ditto	37	,,	× ×	×	×	× ×	
119	Healthy breath	,,	,,	:	×			
121	Ditto	22	,,	× ×	× × ×	× ×	× ×	
118	Ditto	,,	,,	× ×	× × ×	× × ×	× × ×	
120	Ditto	37	,,	×	× ×	× ×	× ×	
131	Pure ground air	,,	,,	×	× ×	× ×	× ×	
132	Ditto	,,	,,	×	× ×	× × ×	× × ×	
133	Ditto	,,	,,	×	× ×	×	× ×	
135	Ditto	,,	,,	×	× ×	×	× ×	

Table IX.—Control Cultivations.

No.	Media.	Temperature.	Date of inoculation.	Periods of cultivation.				Remarks.
				2 weeks.	4 weeks.	2 months.	3 months.	
113	Blood serum	37° C.	Feb. 10	×	× ×	× ×	× ×	
114	Agar peptone	„	„	×	×	×	×	
136	Blood serum	22	Mar. 2	×	× ×	× × ×	× × ×	
137	Ditto	37	„	: :	: :	×	×	
138	Agar peptone	22	„	: ×	×	×	× ×	
139	Ditto	„	„	: :	×	×	× ×	
140	Gelatine peptone	„	„				×	
141	Ditto	„	„	× ?	× ×	× ×	× ×	
142	Potato tubes	37	„	×	× ×	× × ×	× × ×	
143	Ditto	„	„	: :	×	×	×	
144	Ditto	22	„				×	
145	Ditto	„	„					

It will be seen that in many of the tubes a free growth was observed as early as the end of the first fortnight.

Out of the total number in this series of 37, in thirty-six instances there was free growth on the medium employed, on both kinds of paper, and all kinds of condensed fluid. Eleven of them were grown at a temperature of about 20° C. In only one instance was there complete failure (vapour from healthy breath).

Most of these tubes have been left intact, in order that they may be inspected; but six of them were removed, stained, and examined microscopically, in order to determine whether they were true cultures; this they proved to be.

Two of the cultures, after two months' growth, were sent away to be inoculated into guinea-pigs, but both they and the original culture were found to be non-virulent.*

Microscopic Examination.

Nearly all the earlier cultures, in which there appeared to have been any growth, were submitted to microscopical examination. In all the specimens in which this examination did not show distinct signs of growth the result was put down as "nil," even though a small number of bacilli might have been found. These few bacilli might have come from the inoculation. It was not difficult to recognise the abundant growth of a true cultivation.

These examinations, however, gave remarkable results in a large number of the specimens grown upon paper. Many of the bacilli were gigantic in size, and a considerable number of them showed distinct branching. Others were knobbed at one end or at both ends, when they looked like miniature "life preservers." In many of the specimens the culture seemed to have penetrated into the substance of the paper.

The bearing of these researches upon the subject of the prophylaxis against tuberculosis seems to be of some importance.

They prove that any one of the various organically charged vapours, whether coming from healthy or from diseased lungs, from the air of cellars, or from comparatively pure ground, forms an excellent cultivating medium for the bacillus of tubercle when kept away from the disinfecting influence of air and light.

This power of promoting its growth is particularly manifest when the supporting substance is common wall-paper, though it is quite apparent when very pure filter-paper is used.

It is further proved that, on these substances, the growth of the bacillus may take place at the ordinary temperatures of dwelling-

* A further research, with cultures of the bacillus of undoubted virulence, has now been undertaken.

rooms; and, hence, that there is no safety against the increase of the
organism in ordinary living rooms in which active tuberculous dust
is present, and in which the natural disinfectants of the bacillus,
fresh air and light, are not present in sufficient amount to destroy
their virulence.

" Summary of Professor Edgeworth David's Preliminary Report
    on the Results of the Boring in the Atoll of Funafuti."
    Communicated by Professor T. G. BONNEY. F.R.S., Vice-
    Chairman of the Coral Reef Boring Committee.   Received
    November 25,—Read November 25, 1897.

The boring at Funafuti, according to the latest advices, had reached
a depth of 643 feet.   Professor David's report is transcribed from
notes made during the progress of the work, and gives his first im-
pressions of the materials brought up, down to a depth of 557 feet,
which had been reached when he quitted the island to return to his
duties at Sydney, leaving the work in charge of his assistant.   The
latest advices informed him that the boring was arrested at 643 feet,
but as it was hoped this was only for a time. we are daily expecting
to hear yet more gratifying news.   His last letters, received during
the present week, give a few particulars of the materials pierced
between 557 and 643 feet.   The work, Professor David states, often pre-
sented most serious difficulties, which would probably have frustrated
their efforts, but for the experience gained on the former occasion.

The bore hole is situated about half a mile N.E. of the Mission
Church, and its height above sea level is about 1 foot above high
water mark at spring tides.   The diameter is 5 inches down to
68 feet; it is lined with 5-inch tubing down to 118 feet, and 4-inch
from surface to 520 feet, so that on September 6 a 4-inch core was
being obtained.

The following is a general description of the materials pierced :—
For about a yard at the top there was a hard coral breccia.   This
was followed down to a depth of 40 feet by "coral reef rock,"
into the composition of which *Heliopora cerulea*, with spines of
echinids and nullipores, entered largely, the last predominating over
the coral at from 15 to 20 feet.   From 40 to 200 feet came more or
less sandy material, but with a variable quantity of corals.   These
were scattered through the sand (calcareous and of organic origin;
foraminifera, at about 40 feet, making from one-half to two-thirds of
the whole) sometimes as fragments (forming occasionally a kind
of rubble), but sometimes in the position of growth.   Between
120 and 130 feet, and from about 190 to 200 feet, the material

is described as fairly compact coral rock, so that very probably reefs
in situ, though of no great thickness, were pierced at these depths.
The sand appears to be largely derived from coral, but foraminifera
occur, sometimes in abundance; so too do nullipores, and here and
there spines of echinids. Towards 150 feet signs of change begin
to appear in the corals, and these become more conspicuous as the
boring approaches its greatest depth. In such case, if I understand
rightly, some of the branching corals crumble away and are repre-
sented only by casts, while others remain, the surrounding matrix
becoming solid, cemented apparently by calcite. Below 202 feet a
decided change takes place in the character of the deposit. All
above this seems to be largely composed of material derived from
corals, with occasional rather brief interludes of true reef, and this
mass, measuring, as said above, rather over 200 feet in thickness,
may be termed the first or uppermost formation. Below this, down to
about 373 feet, sandy material distinctly dominates, which sometimes is
almost a calcareous mud. Still even there coral fragments and rubble
occasionally appear, and now and then a few isolated corals. Other
organisms may be detected, including nullipores, foraminifera, and
mollusca; but until this material has been examined microscopically,
it would be premature to attempt any precise statement. This mass,
in thickness about 170 feet, may be termed the second or middle
formation. It is not reef, though obviously produced in the vicinity
of a reef. Below 370 feet is the third or lowest zone; in this beds
composed of broken coral become frequent, which are intercalated
with masses of dead coral, though sandy bands also occur. The
character of the material suggests that it has been formed in the
immediate vicinity of a reef, which has occasionally grown out
laterally, though only for a time, and has built up a layer of true
reef, from 2 to 3 feet in thickness, upon a mass of detrital coral.
In one place the rock is specially noted as " hard," and hereabouts
even the shells of gasteropods have perished, only their casts remain-
ing. From 526 to 555 feet the bore passed through fairly compact
and (in places) very dense and hard " coral limestone " and " cav-
ernous coral rock," in which dendroid forms were numerous. As
regards the part between 557 feet and 643 feet only brief infor-
mation is to hand, but Professor David states that it is reported to be
chiefly coral limestone, hard and dense, with occasional soft bands of
coral sand or coral rubble. Thus the third, or lowest zone, about
270 feet in thickness, corresponds apparently with the first, but it
seems to contain larger and more numerous masses of true reef.

Professor David has also forwarded with his latest letters a section
of the boring and of the exterior form of the island, down to about
730 fathoms : the one drawn from his notebook, the other from
Captain Field's record of soundings. From this I gather the

following particulars :—The borehole is, roughly speaking, rather over 100 yards from the margin of the ocean, and about 165 yards from that of the lagoon; it is about 240 yards from the spot where a sounding of 10 fathoms was obtained, nearly 400 yards from a 36-fathom sounding, and rather more than a quarter of a mile from one of 130 fathoms. After this the submarine slope, for a considerable depth, is not quite so steep. He also states that, at Funafuti, the vigorous growing portion of the reef appeared to be limited to within about 40 feet of the surface.

It would be premature, as Professor David remarks, to express an opinion as to the theoretical bearing of these results until the core has been thoroughly studied. But two things seem clear, (1) that true reef has been pierced at depths down to more than 600 feet, and (2) that throughout the whole of the time represented by the mass which has now been tested, coral must have grown in great abundance in some part or other of the locality now represented by Funafuti; for the atoll, it must be remembered, is surrounded by water about 2,000 fathoms deep, what would completely isolate it from any other coralliferous locality.

November 30, 1897.

Anniversary Meeting.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A full Report of the Anniversary Meeting, with the President's Address and Report of Council, will be found in the 'Year-book' for 1897–8.

The Account of the Appropriation of the Donation Fund and of the Government Grant will also be found in the 'Year-book.'

December 9, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had nominated as Vice-Presidents for the ensuing year—

> The Treasurer (Sir John Evans).
> Professor Clifton.
> Professor Story Maskelyne.
> Dr. W. J. Russell.

The following Papers were read:—

I. "On the Densities of Carbonic Oxide, Carbonic Anhydride, and Nitrous Oxide." By LORD RAYLEIGH, F.R.S.

II. "On the Application of Harmonic Analysis to the Dynamical Theory of the Tides. Part II. On the General Integration of Laplace's Dynamical Equations." By S. S. HOUGH, M.A., Fellow of St. John's College, and Isaac Newton Student in the University of Cambridge. Communicated by Professor G. H. DARWIN, F.R.S.

III. "A Note on some further Determinations of the Dielectric Constants of Organic Bodies and Electrolytes at very Low Temperatures." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London.

IV. "On Methods of making Magnets independent of Changes of Temperature; and some Experiments upon Negative Temperature Coefficients in Magnets." By J. REGINALD ASHWORTH. Communicated by ARTHUR SCHUSTER, F.R.S.

V. "The Electric Conductivity of Nitric Acid." By V. H. VELEY, M.A., F.R.S., and J. J. MANLEY, Daubeny Curator of the Magdalen College Laboratory, Oxford.

VI. "On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix, and of the Electromagnetic Force between a Helical Current and a uniform Coaxial Circular Cylindrical Current Sheet." By J. VIRIAMU JONES, F.R.S.

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VII. "On the Refractivities of Air, Oxygen, Nitrogen, Argon,
    Hydrogen, and Helium." By Professor WILLIAM RAMSAY,
    Ph.D., LL.D., Sc.D., F.R.S., and MORRIS W. TRAVERS, B.Sc.

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"On the Densities of Carbonic Oxide, Carbonic Anhydride, and
    Nitrous Oxide." By LORD RAYLEIGH, F.R.S.   Received
    October 12,—Read December 9, 1897.

The observations here recorded were carried out by the method
and with the apparatus described in a former paper,* to which
reference must be made for details.   It must suffice to say that the
globe containing the gas to be weighed was filled at 0° C., and to a
pressure determined by a manometric gauge.   This pressure, nearly
atmospheric, is slightly variable with temperature on account of
the expansion of the mercury and iron involved.   The actually
observed weights are corrected so as to correspond with a tempera-
ture of 15° C. of the gauge, as well as for the errors in the platinum
and brass weights employed.   In the present, as well as in the
former, experiments I have been ably assisted by Mr. George
Gordon.

Carbonic Oxide.

This gas was prepared by three methods.   In the first method a
flask, sealed to the rest of the apparatus, was charged with 80 grams
recrystallised ferrocyanide of potassium and 360 c.c. strong sulphuric
acid.   The generation of gas could be started by the application of
heat, and with care it could be checked and finally stopped by the
removal of the flame with subsequent application, if necessary, of
wet cotton wool to the exterior of the flask.   In this way one charge
could be utilised with great advantage for several fillings.   On
leaving the flask the gas was passed through a bubbler containing
potash solution (convenient as allowing the rate of production to be
more easily estimated) and thence through tubes charged with frag-
ments of potash and phosphoric anhydride, all connected by sealing.
When possible, the weight of the globe *full* was compared with the
mean of the preceding and following weights *empty*.   Four experi-
ments were made with results agreeing to within a few tenths of a
milligram.

In the second set of experiments the flask was charged with
100 grams of oxalic acid and 500 c.c. strong sulphuric acid.   To
absorb the large quantity of CO_2 simultaneously evolved a plentiful

* "On the Densities of the Principal Gases," 'Roy. Soc. Proc.,' vol. 53, p. 134,
1893.

supply of alkali was required. A wash-bottle and a long nearly horizontal tube contained strong alkaline solution, and these were followed by the tubes containing solid potash and phosphoric anhydride as before.

For the experiments of the third set *oxalic* acid was replaced by *formic*, which is more convenient as not entailing the absorption of large volumes of CO_2. In this case the charge consisted of 50 grams formate of soda, 300 c.c. strong sulphuric acid, and 150 c.c. distilled water. The water is necessary in order to prevent action in the cold, and the amount requires to be somewhat carefully adjusted. As purifiers, the long horizontal bubbler was retained and the tubes charged with solid potash and phosphoric anhydride. In this set there were four concordant experiments. The immediate results stand thus :—

<div align="center">

Carbonic Oxide.

</div>

From ferrocyanide	2·29843
„ oxalic acid	2·29852
„ formate of soda	2·29854
Mean............	2·29850

This corresponds to the number 2·62704 for oxygen,[*] and is subject to a correction (additive) of 0·00056 for the diminution of the external volume of the globe when exhausted.

The ratio of the densities of carbonic oxide and oxygen is thus 2·29906 : 2·62760; so that if the density of oxygen be taken as 32, that of carbonic oxide will be 27·9989. If, as some preliminary experiments by Dr. Scott[†] indicate, equal volumes may be taken as accurately representative of CO and of O_2, the atomic weight of carbon will be 11·9989 on the scale of oxygen $= 16$.

The very close agreement between the weights of carbonic oxide prepared in three different ways is some guarantee against the presence of an impurity of widely differing density. On the other hand, some careful experiments led Mr. T. W. Richards[‡] to the conclusion that carbonic oxide is liable to contain considerable quantities of hydrogen or of hydrocarbons. From $5\frac{1}{2}$ litres of carbonic oxide passed over hot cupric oxide he collected no less than 25 milligrams of water, and the evidence appeared to prove that the hydrogen was really derived from the carbonic oxide. Such a proportion of hydrogen would entail a deficiency in the weight of the globe of about 11 milligrams, and seems improbable in view of the good agreement of the numbers recorded. The presence of so much

[*] " On the Densities of the Principal Gases," 'Roy. Soc. Proc.,' vol. 53, p. 144.
[†] ' Camb. Phil. Proc.,' vol. 9, p. 144, 1896.
[‡] ' Amer. Acad. Proc.,' vol. 18, p. 279, 1891.

hydrogen in carbonic oxide is also difficult to reconcile with the well-known experiments of Professor Dixon, who found that prolonged treatment with phosphoric anhydride was required in order to render the mixture of carbonic oxide and oxygen inexplosive. In the presence of relatively large quantities of free hydrogen (or hydrocarbons) why should traces of water vapour be so important?

In an experiment by Dr. Scott,* 4 litres of carbon monoxide gave only 1·3 milligrams to the drying tube after oxidation.

I have myself made several trials of the same sort with gas prepared from formate of soda exactly as for weighing. The results were not so concordant as I had hoped,† but the amount of water collected was even less than that given by Dr. Scott. Indeed, I do not regard as proved the presence of hydrogen at all in the gas that I have employed.‡

Carbonic Anhydride.

This gas was prepared from hydrochloric acid and marble, and after passing a bubbler charged with a solution of carbonate of soda, was dried by phosphoric anhydride. Previous to use, the acid was caused to boil for some time by the passage of hydrochloric acid vapour from a flask containing another charge of the acid. In a second set of experiments the marble was replaced by a solution of carbonate of soda. There is no appreciable difference between the results obtained in the two ways; and the mean, corrected for the errors of weights and for the shrinkage of the globe when exhausted, is 3·6349, corresponding to 2·6276 for oxygen. The temperature at which the globe was charged was 0° C., and the actual pressure that of the manometric gauge at about 20°, reduction being made to 15° by the use of Boyle's law. From the former paper it appears that the actual height of the mercury column at 15° is 762·511 mm.

Nitrous Oxide.

In preliminary experiments the gas was prepared in the laboratory, at as low a temperature as possible, from nitrate of ammonia, or was drawn from the iron bottles in which it is commercially supplied. The purification was by passage over potash and phosphoric anhydride. Unless special precautions are taken the gas so obtained is ten or more milligrams too light, presumably from admixture with

* 'Chem. Soc. Trans.,' 1897, p. 564.

† One obstacle was the difficulty of re-oxidising the copper reduced by carbonic oxide. I have never encountered this difficulty after reduction by hydrogen.

‡ In Mr. Richards' work the gas in an imperfectly dried condition was treated with hot platinum black. Is it possible that the hydrogen was introduced at this stage?

nitrogen. In the case of the commercial supply, a better result is obtained by placing the bottles in an inverted position so as to draw from the *liquid* rather than from the *gaseous* portion.

Higher and more consistent results were arrived at from gas which had been specially treated. In consequence of the high relative solubility of nitrous oxide in water, the gas held in solution after prolonged agitation of the liquid with impure gas from any supply, will contain a much diminished proportion of nitrogen. To carry out this method on the scale required, a large (11-litre) flask was mounted on an apparatus in connection with the lathe so that it could be vigorously shaken. After the dissolved air had been sufficiently expelled by preliminary passage of N_2O, the water was cooled to near 0° C. and violently shaken for a considerable time while the gas was passing in large excess. The nitrous oxide thus purified was expelled from solution by heat, and was used to fill the globe in the usual manner.

For comparison with the results so obtained, gas purified in another manner was also examined. A small iron bottle, fully charged with the commercial material, was cooled in salt and ice and allowed somewhat suddenly to blow off half its contents. The residue drawn from the bottle in one or other position was employed for the weighings.

<div align="center">Nitrous Oxide (1896).</div>

Aug. 15	Expelled from water	....		3·6359
„ 17	....			3·6354
„ 19	From residue after blow off, valve downwards			3·6364
„ 21	„	„	valve upwards..	3·6358
„ 22	„	„	valve downwards	3·6360
	Mean......................			3·6359

The mean value may be taken to represent the corrected weight of the gas which fills the globe at 0° C. and at the pressure of the gauge (at 15°), corresponding to 2·6276 for oxygen.

One of the objects which I had in view in determining the density of nitrous oxide was to obtain, if it were possible, evidence as to the atomic weight of nitrogen. It may be remembered that observations upon the density of pure nitrogen, as distinguished from the atmospheric mixture containing argon which, until recently, had been confounded with pure nitrogen, led[*] to the conclusion that the densities of oxygen and nitrogen were as 16 : 14·003, thus suggesting that the atomic weight of nitrogen might really be 14 in place of 14·05, as generally received. The chemical evidence upon which the latter number rests is very indirect, and it appeared that a direct compari-

* Rayleigh and Ramsay, ' Phil. Trans.,' vol. 186, p. 190, 1895.

son of the weight of nitrous oxide and of its contained nitrogen might be of value. A suitable vessel would be filled, under known conditions, with the nitrous oxide, which would then be submitted to the action of a spiral of copper or iron wire rendered incandescent by an electric current. When all the oxygen was removed, the residual nitrogen would be measured, from which the ratio of equivalents could readily be deduced. The fact that the residual nitrogen would possess nearly the same volume as the nitrous oxide from which it was derived would present certain experimental advantages. If indeed the atomic weights were really as 14 : 16, the ratio (x) of volumes, after and before operations, would be given by

$$\frac{2\cdot2996 \times x}{3\cdot6359 - 2\cdot2996 \times x} = \frac{14}{8},$$

whence $\qquad x = \frac{7 \times 3\cdot6359}{11 \times 2\cdot2996} = 1\cdot0061,$

3·6359 and 2·2996 being the relative weights of nitrous oxide and of nitrogen which (at 0° C. and at the pressure of the gauge) occupy the same volume. The integral numbers for the atomic weights would thus correspond to an expansion, after chemical reduction, of about one-half per cent.

But in practical operation the method lost most of its apparent simplicity. It was found that copper became unmanageable at a temperature sufficiently high for the purpose, and recourse was had to iron. Coils of iron suitably prepared and supported could be adequately heated by the current from a dynamo without twisting hopelessly out of shape; but the use of iron leads to fresh difficulties. The emission of carbonic oxide from the iron heated in vacuum continues for a very long time, and the attempt to get rid of this gas by preliminary treatment had to be abandoned. By final addition of a small quantity of oxygen (obtained by heating some permanganate of potash sealed up in one of the leading tubes) the CO could be oxidised to CO_2, and thus, along with any H_2O, be absorbed by a lump of potash placed beforehand in the working vessel. To get rid of superfluous oxygen, a coil of incandescent copper had then to be invoked, and thus the apparatus became rather complicated.

It is believed that the difficulties thus far mentioned were overcome, but nevertheless a satisfactory concordance in the final numbers was not attained. In the present position of the question no results are of value which do not discriminate with certainty between 14·05 and 14·00. The obstacle appeared to lie in a tendency of the nitrogen to pass to higher degrees of oxidation. On more than one occasion mercury (which formed the movable boundary of an overflow chamber) was observed to be attacked. Under these circumstances

I do not think it worth while to enter into further detail regarding the experiments in question.

The following summary gives the densities of the various gases relatively to air, all obtained by the same apparatus.* The last figure is of little significance.

<div style="margin-left:2em">

Air free from H_2O and CO_2 .... 1·00000
Oxygen .... 1·10535
Nitrogen and argon (atmospheric) .... 0·97209
Nitrogen........................... 0·96737
Argon............................. 1·37752
Carbonic oxide .... 0·96716
Carbonic anhydride .... 1·52909
Nitrous oxide .... 1·52951

</div>

The value obtained for hydrogen upon the same scale was 0·06960; but the researches of M. Leduc and of Professor Morley appear to show that this number is a little too high.

"On the Application of Harmonic Analysis to the Dynamical Theory of the Tides. Part II. On the general Integration of Laplace's Dynamical Equations." By S. S. Hough, M.A., Fellow of St. John's College, and Isaac Newton Student in the University of Cambridge. Communicated by Professor G. H. Darwin, F.R.S. Received October 27, —Read December 9, 1897.

(Abstract.)

The former part of this paper deals with solutions of Laplace's differential equations for the tides symmetrical with respect to the axis of rotation. In the present part the restriction of symmetry is no longer imposed, and a general solution is sought, the law of depth of the ocean, however, being limited to the case which will admit of both the interior and exterior surfaces being regarded as spheroids of revolution. It is found that, subject to this limitation, if the solution sought represent a simple harmonic motion of any period whatsoever, and the height of the surface-waves be expressible as an infinite series of tesseral harmonics of the same rank but different orders, a linear relation connecting three successive coefficients of the series can be deduced similar to that obtained in Part I.†

From this relation a period-equation for the free vibrations is deduced, and a method of determining approximate values of the

* ' Roy. Soc. Proc.,' vol. 53, p. 148, 1893 ; vol. 55, p. 340, 1894 ; ' Phil. Trans.,' vol. 186, p. 189, 1895 ; ' Roy. Soc. Proc.,' vol. 59, p. 201, 1896.

† ' Roy. Soc. Proc.,' vol. 61, p. 236.

higher roots is given. The earlier roots are examined numerically, and tabulated for four different depths of the ocean for the types involving tesseral harmonics of rank 1 and 2, these types presenting special interest in connection with the diurnal and semi-diurnal forced tides respectively.

The types of free oscillation are found to be of two classes, distinguishable by their limiting forms when the rotation-period is indefinitely prolonged. In the former class the motion remains oscillatory when the period of rotation becomes infinitely long, while in the latter the "speed" of the oscillation always bears a finite ratio to the angular velocity of rotation, so that the oscillation will be replaced by a steady motion when the angular velocity of rotation is reduced to zero.

In dealing with the forced oscillations, the theorem of Laplace that in an ocean of uniform depth there will be no diurnal rise and fall at the surface is obtained and generalised as follows :—In an ocean of uniform depth the tides due to a disturbing potential of degree $s+1$ and rank s will involve no rise and fall at the surface if the period of the disturbing force be $\frac{1}{2}(s+1)$ sidereal days.

A theorem given by Professor Darwin with reference to the expression of the semi-diurnal tides in finite terms, as also Laplace's solution of the problem of the diurnal tides in an ocean of variable depth, is found to admit of similar generalisation.

The general problem of the forced tides due to any disturbing force derivable from a potential function in the cases where infinite series are required for the solution is treated analytically, and is further illustrated by numerical examples typical of the leading tidal constituents which occur on the earth, the results where possible being compared with those obtained by other methods.

"On Methods of making Magnets independent of Changes of Temperature; and some Experiments upon Abnormal or Negative Temperature Coefficients in Magnets." By J. REGINALD ASHWORTH, B.Sc. Communicated by ARTHUR SCHUSTER, F.R.S. Received October 29,—Read December 9, 1897.

The present investigation, which has been carried out in the Physical Laboratory of the Owens College, Manchester, was undertaken at Professor Schuster's suggestion with the object of ascertaining what kinds of iron and steel are least liable to a change of magnetic intensity under moderate fluctuations of temperature.

Specimens of steels containing severally tungsten, manganese, cobalt, and nickel, also cast irons, of different blends of pig irons, and

of different percentages of carbon, were procured from a number of different English and Scotch firms. The size of these specimens was in general about 15 cm. long and 1 or 2 thick, but as it was not uniform the dimensions and weight in grams of each are given in the accompanying table, columns I, II, and III.

In column IV has been entered the dimension ratio, *i.e.*, the ratio of the length to the diameter or breadth, so that a comparison may more consistently be made of the magnetic behaviour of any two specimens. For thin rods, Cancani* finds that increase of this ratio tends to diminish the temperature coefficient of a magnet.

The course of an experiment was as follows:—The rod or bar in its normal state, or after being hardened or annealed as occasion required, was magnetised between the poles of a powerful electro-magnet excited by a battery of twenty-six storage cells. The magnet was then fixed rigidly in a horizontal tube, through which a stream of cold water and steam could be alternately passed. The tube and its contents were placed at a convenient distance from a sensitive dead-beat magnetometer and at right angles to the magnetic meridian. The deflections of the magnetometer needle were read by the usual mirror and scale, the distance of the scale from the mirror being 1 metre, and from the readings were deduced directly the tempera-ture coefficient and the total irreversible loss of magnetism. As the deflections were never more than a few degrees of arc the angles and their tangents were virtually equivalent. The intensity of magneti-sation in C.G.S. units or magnetic moment per unit volume, although not necessarily required, was approximately determined from the formula

$$I = H \frac{(d^2 - l^2)^2}{2d} \tan \theta \, \frac{\sigma}{m},$$

in which the earth's horizontal force, H, was considered throughout as constant and equal to 0·18 C.G.S. unit and also σ, the density, was uniformly taken to be 7·8; m signifies the mass in grams; d the distance from the centre of the magnet to the magnetometer needle; l the half length of the magnet, and θ the deflection.

The process of heating and cooling the magnet was continued until the intensity fluctuated between two nearly constant values corre-sponding to the temperatures of the cold water and steam. The coefficient α given in the eighth column was then calculated by inserting these values in the equation

$$I_{t'} = I_t (1 - \alpha \overline{t' - t}).†$$

* R. Cancani, 'Atti della R. Acc. dei Lincei,' (4), 3, pp. 501—506, 1887; 'Beibl.,' vol. 11, 1887.

† I have followed the customary mode of writing this formula with a negative sign preceding the coefficient, a; and, hence, a *negative coefficient* indicates an *increase* of magnetic intensity with *increase* of temperature.

The irreversible loss of original magnetic intensity which results from a series of heatings and coolings is tabulated under the heading β in column VII, β being calculated from the formula

$$I_f = I_i (1 - \beta),$$

where I_f and I_i are the final and initial intensities.

The limits of temperature t and t' in these experiments were 10° to 20° C. and about 100° C., giving a range of 80° or 90°.

The centigrade scale of temperatures and the C.G.S. system of units are to be understood throughout.

In every case a record has been kept of the scale readings at the temperatures t and t' during the progress of the operations of heating and cooling, and the brief example here cited may be taken as typical.

Three per cent. Tungsten Steel.

| Temperature. | Scale readings at | | Zero   0·0 |
	t.	t'.	
6·5	184·8		
99·6	..	142·6	
7·5	161·3		
99·6	..	140·2	
7·5	160·2		
99·6	..	137·9	
7·5	158·8		
99·6	..	137·9	
7·5	158·6		Zero −0·1

The table which is annexed gives a synopsis of the results obtained.

Each number in the first column represents a separate piece of iron or steel, but where comparative tests of the same material were desired, as, for example, in the cast irons, when either annealed or hardened, the precaution was taken to employ two pieces of originally a single rod. Thus Nos. 15 and 16 are two parts of the same rod cut through the middle, and similarly with Nos. 17 and 18 and others.

In the first place, several varieties of steels were tested. No. 1 is a steel from Sheffield, supplied specially for making magnets; Nos. 2 and 3 are Hadfield's well-known non-magnetic steel; the next four are tungsten steels, of which Nos. 6 and 7 are known as Mushet's self-hardening steel, having the property of hardening even when cooled slowly. These were both cut from the same rod; No. 6 was

Table I.

No.	Specimen.	Condition.	I. 2l.	II.* d.	III. m.	IV. R = 2l/d.	V. I_i.	VI. I_f.	VII. β.	VIII. α 0·00.	
1	"Magnet" steel.	Hardened	15·9	2·0	366	8·0	53·2	40·6	0·237	+·137	From Sheffield.
2	Manganese steel.	,,	15·2	2·4	532	6·3	0·75	0·63	0·150	+·031	Do.
3	Tungsten steel...	Annealed	15·1	2·3	520	6·5	0·30	0·257	0·150	+·045	Do.
4		Hardened	16·0	2·6(s)	855	6·7	39·7	34·1	0·142	+·142	W = 3 p. c. C = 0·6 to 1·0 p. c. From Sheffield.
5	,,	,,	16·0	2·7(s)	856	5·9	69·8	50·3	0·280	+·025	W = 6·1 p. o. C = 2·1 p. c. Mn = 1·8 p. c. Cr = 0·5 p.c. Si = 0·5 p. c. From Sheffield.
6	,,		11·9	0·97	65	12·0	185·9	132·6	0·287	+·069	Mushet's self-hardening steel.
7	,,		16·5	0·97	92	17·0	215·3	177·8	0·174	+·097	Do.
8	Cobalt steel.....		10·8	3·14	650	3·4	16·4	12·8	0·217	+·115	Do.
9	Nickel steel.....		16·0	2·5(s)	777	6·4	28·7	24·1	0·160	+·025	Co = 4·5 p. c. C = 0·5 p.c. Ni = 3 p. c. C = 0·45 p. c. Do.
10		As supplied	20·3	1·95	430	10·4	16·7	12·6	0·244	+·200	Ni = 2·4 p. c. C = 0·19 p. c. Scotch.
11		Hardened	20·3	1·95	480	10·4	128·0	109·4	0·145	+·032	
12		,,	15·7	1·6(s)	331	9·8	79·8	35·7	0·526	-·017	
		Annealed	,,	,,	,,	,,	106·4	66·1	0·303	+·024	Ni = 3 p. c. C about 0·22 p. c. Do.
13		Hardened	16·0	1·4(s)	247	11·4	111·7	55·9	0·500	-·018	
14		,,	15·8	1·3(s)	242	12·5	0·54	0·42	0·225	-·054	
15		,,	17·5	1·35	175	13·0	0·54	0·45	0·169	-·005	Ni = 27 p. c. C about 0·27 p. c. Do.
16	Cast iron......	As supplied	16·0	1·35	160	11·8	52·1	37·6	0·279	+·288	Dalmellington, Yorkshire, and scrap irons.
17		Hardened	15·3	1·17	113	13·1	193·1	167·8	0·131	+·018	
18		As supplied	15·3	1·17	111	13·1	59·5	37·2	0·374	+·272	Glengarnoch, and scrap irons.
19		Hardened	15·3	1·17	113	13·1	210·9	180·1	0·146	+·016	
20		As supplied	15·2	1·17	112	13·0	56·9	35·6	0·374	+·242	Scotch, white, and scrap irons.
21		Hardened	15·2	1·17	112	13·0	190·6	174·8	0·083	+·016	
22		As supplied	12·8	0·98	65	12·5	27·3	26·3	0·038	+·053	Carbon over 5 per cent.
23		Hardened	12·0	0·96	59	12·5	31·1	27·9	0·104	+·029	
		,,	13·6	2·52	473	5·4	5·0	4·6	0·074	+·054	

* (s) signifies that the cross section of the specimen is square; the dimensional ratio of such is figured in *italics.* All others had a circular cross section.

magnetised at the air temperature; No. 7 was made red hot and allowed to cool whilst in the magnetic field. No. 8, a specimen of cobalt steel, was kindly supplied by Mr. Hadfield, and is probably unique. All of these and the first example of nickel steel, No. 9 on the list, are Sheffield steels.

Attention was then directed chiefly to three classes: Nickel steels, cast irons, and steel pianoforte wires.

Nickel Steels.—The first of these, No. 9, is a crucible steel from Sheffield, containing 3 per cent. of nickel and about 0·45 per cent. of carbon. The next two are from Scotland. They contain 2·4 per cent. of nickel and 0·19 per cent. of carbon. Nos. 12, 13, and 14 are also from Scotland, and contain 3 per cent. and 27 per cent. of nickel. They were kindly supplied by Mr. Riley, of the Glasgow Iron and Steel Company. The behaviour of the last three was remarkable, as when hardened they exhibited a small, *negative* coefficient. On heating and cooling they continuously lost magnetism for the first three alternations; at the fourth and fifth heating and cooling there was hardly any change of intensity; afterwards a small increase of intensity with rise of temperature and decrease with fall of temperature regularly took place. In the specimen containing 3 per cent. of nickel these operations caused a total loss of no less than 50 per cent. of the original magnetic intensity. This same piece was then annealed and magnetised; the coefficient was now *positive*, the intensity rather higher, and the total loss 30 per cent. On re-hardening the events first described were reproduced, the negative coefficient and large total loss being almost exactly as before. It is very likely that by carefully adjusting the degree of hardness in this kind of steel a zero coefficient could be obtained.

The 27 per cent. nickel alloys, after hardening in cold water, became almost non-magnetic, as discovered by Dr. John Hopkinson,[*] and it was only in this state that they were tested. No. 13 was magnetised at the air temperature; No. 14 at −16°. All the other examples of nickel steels had positive coefficients.

Cast Irons.—Specimens of grey cast iron, as used for general castings made at different times and of different blends of pig irons behaved very similarly. Magnetised as supplied they did not take a high intensity, lost permanently 30 to 40 per cent. of their magnetism, and had a large temperature coefficient. When hardened their magnetic properties were very different; the intensity was then comparable with that of tungsten steel, the total loss only about 15 per cent., and the temperature coefficient as low as, or lower than, the best examples of hardened steels. In three different kinds of carefully hardened cast-iron magnets it was from 0·00016 to 0·00018 per degree centigrade. The average value for steel magnets of a similar

[*] Hopkinson, 'Roy. Soc. Proc.,' vol. 48, p. 61.

size tested at the Kew Observatory is given by Whipple as 0·00029.[*]
The change of intensity with temperature is almost strictly linear in
these cast-iron magnets, and they are very constant when subjected
to blows and shocks.

Pianoforte Wire.—Lengths of 12 cm. each were cut from a coil of
wire, and tested after various treatments. Magnetised in the normal
state this material unexpectedly gave a *negative* coefficient. When
heated to bright redness and chilled rapidly or slowly the coefficient
became positive.

As it was thus possible to change the sign of the coefficient, an
attempt was made to find the particular temper which would give a
zero coefficient. Lengths of the wire were heated severally in oil to
200° and 260°, and in air to a temperature producing a film of oxide,
and rapidly chilled in water. The coefficient still remained negative,
and of nearly the same magnitude. But when heated to dull redness
and quenched, the coefficient was very nearly reduced to zero.
Heated to higher temperatures and quenched, the coefficient became
positive.

Table II.

No.	Condition.	$R = 2l/d$.	I_i.	I_f.	β.	$\propto 0\cdot00$.
16*a*	As supplied	109	649·0	644·6	0·008	−0·023
	Tempered at 260°..	,,	792·1	769·2	0·029	−0·018
	Ditto dull red	,,	883·0	863·6	0·022	−0·002
	Ditto, ditto.......	,,	892·0	869·0	0·026	+0·003
	Glass hard	,,	559·5	537·1	0·040	+0·008
	Annealed.........	,,	849·0	830·1	0·023	+0·006
16*b*	As supplied.......	100	679·0	633·6	0·067	−0·055
	Glass hard	,,	593·0	497·0	0·163	−0·017

Length of each piece, 12 cm.; weight, about 0·9 gram; diameter, 16*a* = 0·11 cm.,
　16*b* = 0·12 cm. These two specimens are made from different kinds of steel.

It is a curious coincidence that the intensity of magnetisation
attains a maximum for the condition producing minimum tempera-
ture coefficient, and this maximum has the exceptionally high value
of 892 C.G.S. units.

The fact that the negative coefficient could not be reproduced if
once the wire had been heated above a red-heat indicates that there
is some structure physically imposed upon music wire, perhaps in
the process of drawing, which partly or wholly contributes in pro-
ducing the negative coefficient. Whereas the negative coefficient in
the nickel steel is reproducible, and is doubtless a consequence of

[*] Whipple, 'Roy. Soc. Proc.,' vol. 26, p. 218.

intense hardness. In contrast with this it may be mentioned that music wires are not at all hard, being easily touched with a file.

In order to gain further insight into the cause of the negative coefficient in these wires, some experiments were made to test the effect of removing successively the outer layers of the wires by dissolving them in nitric acid. This revealed the important relation that the coefficient became more negative as the diameter became less, the length remaining the same, that is to say, as the dimension ratio increased.

To verify this carefully a series of stout music wires of different thicknesses, but in other respects as uniform as possible, were procured from a manufacturer at Warrington, to whom I am also indebted for kindly supplying other samples of steel wire. The results of these experiments are most conveniently exhibited in tabular form, and are here annexed.

<p align="center">Table III.</p>

No.	$d.$	$m.$	R.	$I_i.$	$I_f.$	$\beta.$	$a.$ 0·00	$a \times d.$ 0·0000
33	0·216	3·535	55·3	530·3	428·5	0·192	−0136	294
30	0·187	2·590	63·8	592·8	508·6	0·142	−0184	344
28	0·174	2·235	69·0	632·4	551·5	0·128	−0226	393
26	0·153	1·760	78·0	736·0	652·8	0·113	−0203	310
24	0·134	1·365	89·0	742·0	686·3	0·075	−0306	410

<p align="center">Length of each piece = 12 cm.</p>

With the exception of No. 26 (and No. 26 was anomalous in some other respects) the coefficients become progressively more negative as the dimension ratio increases. The increasing product of the coefficient into the diameter shows that the coefficient changes more rapidly than the dimension ratio. The table also shows the regular diminution of the permanent loss, β, and increase of intensity as the dimension ratio increases, relations which hold in further experiments of the same kind to be described later on.

Several of these wires after being thus tested were dissolved in nitric acid, and the temperature coefficient determined at successive stages of the process without any remagnetisation of the wire. The results of No. 33 alone are here given, as they sufficiently exemplify what generally takes place under these circumstances. The negative character of the coefficient progresively increases with increase of dimension ratio, and at a rather greater rate as in Table III.

It is interesting to observe in these experiments the increase of

Table IV.

No.	d.	m.	R.	I_i.	I_f.	β.	$\frac{a}{0\cdot00}$.	$\frac{a \times d.}{0\cdot0000}$.
33	0·216	3·535	55·3	530·3	428·5	0·192	−0·0136	294
1st stage	0·195*	2·875	61·3	478·0	464·6	0·028	−0·0155	302
2nd stage	0·163*	1·995	73·6	474·5	468·9	0·012	−0·0196	319
3rd stage	0·112*	0·935	107·5	485·5	482·9	0·005	−0·0292	327

(Dissolved.)

intensity each time the wire is redissolved, remembering that after the initial magnetisation the wire was *not subjected to any further magnetising process.* Thus, for example, No. 33 has an intensity, after being heated and cooled, of 428; upon dissolving off an outer layer the intensity rises to 478, which in its turn is reduced by heatings and coolings to 465; dissolving it a second time raises the intensity to 475, and so on. The recovery of magnetic intensity after dissolving in acid is most likely to be ascribed to diminution of the self-demagnetising force resulting from increase of dimension ratio. The intensities, however, after each dissolving, namely 478, 475, 486, are sufficiently constant to indicate that the intensity is nearly uniform throughout the wire, and this confirms an experiment of Bouty's.†

The next two wires have been grouped in a separate table from the others, as they came from a different factory, being made in Sheffield. They are thicker than the former wires, and the thicker of the two, No. 34, has now a *positive* coefficient. By continually reducing the diameter of this wire, the coefficient ultimately changes sign and becomes *negative.*

Table V.

No.	d.	m.	R.	I_i.	I_f.	β.	$\frac{a.}{0\cdot00}$.
32	0·227	3·875	52·8	490·1	340·6	0·805	−0·0015
34	0·262	5·145	45·8	388·6	271·9	0·304	+0·0220
1st stage ...	0·223‡	3·740	53·7	307·2	299·2	0·026	+0·0193
2nd stage ..	0·204‡	3·130	58·7	297·9	290·3	0·025	+0·0184
3rd stage ..	0·152‡	1·742	78·8	306·6	300·2	0·021	+0·0082
4th stage ..	0·075‡	0·427	159·2	292·5	287·1	0·019	−0·0100

(Dissolved.)

Length, 12 cm.

* Calculated from the weight.
† 'Ann. Scient. de l'Éc. Norm.,' [2], 5, p. 131.
‡ Calculated from the weight.

And it may be calculated that if No. 34 had just been dissolved so far as to have a dimension ratio of about 110 to 115, it would have exhibited a zero coefficient. Since the former series of wires with dimension ratios of this magnitude would have had large negative coefficients, there must be some important physical or chemical differences between these and the former wires influencing the character of the coefficient.

To complete the series of experiments on the influence of the dimension ratio it was desirable to perform the converse operation and to prove that an originally negative coefficient would become positive by increase of thickness.

Three pieces, (a), (b), (c), of No. 33 wire were cut from the same coil, each 12 cm. long, magnetised and then heated and cooled separately in the same way. The coefficient was about —0·000119 for each. (a) and (b) were then bound together with fine copper wire, like poles being in contiguity; the coefficient as now determined was almost zero. The piece (c) was then joined in the same manner to its two fellows and the coefficient again determined; it was now + 0·000105. The experiment is conclusive, for it is allowable to regard bundles of wires as rods of equivalent cross section.*

Wires drawn to different thicknesses are not structurally sufficiently identical to allow of strictly comparable magnetic results. It is therefore more satisfactory to vary the dimension ratio by altering the length and keeping the diameter constant. A series of tests were conducted in this way. Lengths of 3, 6, 9, 12, 15, and . 18 cm. of No. 30 wire were cut from the same coil, separately magnetised, and the coefficient of each very carefully determined. Table VI gives a complete view of the results.

Table VI.

| No.	$2l$.	R.	I_i.	f.	β.	$\dfrac{a.}{0 \cdot 00}$.
30	3 cm.	15·95	137·4	78·7	0·427	+0261
,,	6 ,,	31·90	313·4	204·0	0·349	+0151
,,	9 ,,	47·85	483·2	378·3	0·217	—0084
,,	12 ,,	63·80	602·0	513·8	0·147	—0225
,,	15 ,,	79·75	683·1	595·0	0·129	—0296
,,	18 ,,	95·70	726·8	637·4	0·123	—0317

Diameter of each piece, 0·187 cm.

* Von Waltenhofen, 'Wien. Ber.,' vol. 48, part 2, p. 578, 1863. Ascoli and Lori, ' R. Accad. dei Lincei,' Rome (5), 3, 2 Sem., p. 157, 1894.

The coefficient changes from positive to negative between the lengths 6 and 9 cm. And hence if the change between these points is nearly linear, a length of about 8 cm. should have a zero coefficient, and it might also be calculated that the permanent loss would be 0·262. A fresh length of exactly 8 cm. was cut from the same coil of wire and was found to have a coefficient of −0·000015, and a permanent loss of 0·281. A piece of this wire, a very little less than 8 cm. long, would without doubt, have a strictly zero coefficient.

There are thus two practicable ways of obtaining zero temperature coefficients, either (1) by altering the hardness, or (2) by altering the dimension ratio; and the latter may be effected by varying the diameter for a constant length, or the length for a constant diameter as may be the more convenient. In addition, the material of which the magnet is made must have certain chemical and physical properties, not yet determined, of which, as far as some experiments I have made can decide, the physical rather than the chemical properties are the more important.

Some of the results in Tables IV, V, and VI are here plotted as curves and exhibit interesting features.

The curve of the relation of coefficient to dimension ratio (diameter constant) from the data of Table VI, Diagram I, curve (1), has a double inflexion between which it crosses the axis of abscissæ and
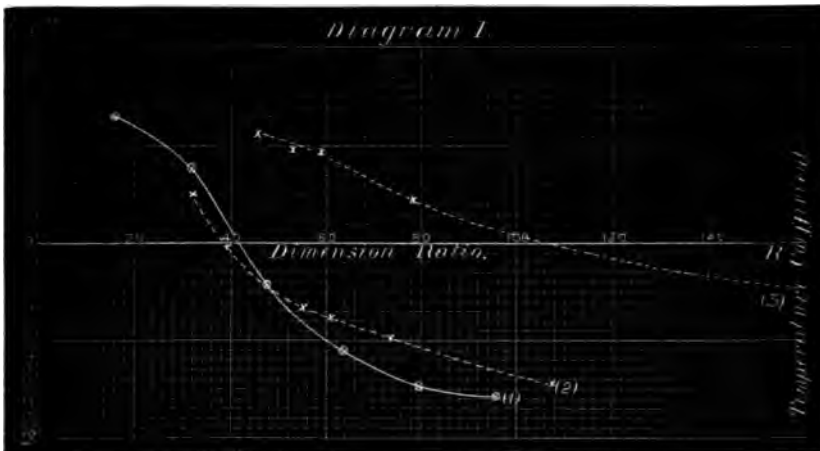
at either end apparently approaches to horizontal asymptotes. This curve is probably typical of the behaviour of music wires.

Curve (2) on this diagram traces the series of experiments on No. 33 wire. The two first points on the left correspond to the

coefficients for three and two pieces bound together, the third point that for a single piece, and succeeding points the coefficients for the same piece at three stages of dissolution. The third curve is constructed from the data in Table V, and represents the passage from a positive to a negative coefficient in No. 34 wire.
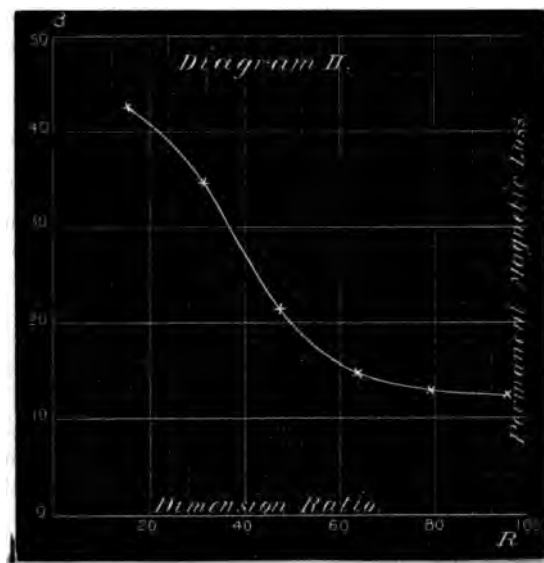

Diagram II exhibits the curve of permanent loss, β, and dimension ratio, R, taken from Table VI for No. 30 wire, diameter constant, and it will be seen it follows remarkably closely the path of the co-efficient curve. The coefficient, α, and the permanent loss, β, may then be connected by a linear equation

$$\alpha = a + b\beta.$$

The values of the constants for this material are

$$a = -0\cdot0005228 \pm 0\cdot0000073 \text{ and}$$

$$b = +0\cdot001886 \pm 0\cdot000043.$$

If curves for α and β be plotted with demagnetising factors, *i.e.*, the demagnetising force per unit intensity, corresponding to their dimension ratios as abscissæ they resemble, strikingly, curves of magnetisation, having a point of inflection near the beginning and ultimately approaching horizontal asymptotes (Diagram III); by prolonging the curves in this diagram until they cut the axis of

ordinates it is easy to estimate what may be called the "character-istic" temperature coefficient and permanent loss for this kind of wire.

It may be inferred that in general the temperature effects upon magnets are principally influenced by the demagnetising factor over a considerable range of dimension ratios, and beyond that range by the nature of the material.

In the fourth diagram the curves of initial and final intensities are plotted with dimension ratios as abscissæ, and they resemble so closely the curve traced in the same way by Barus* for steel of "blue annealed" temper, that it is very probable this is the temper given to the music wires upon which these experiments have been made.

The chief points elicited by this investigation may now be sum-marised :—

1. The temperature coefficient is generally least in the hardest
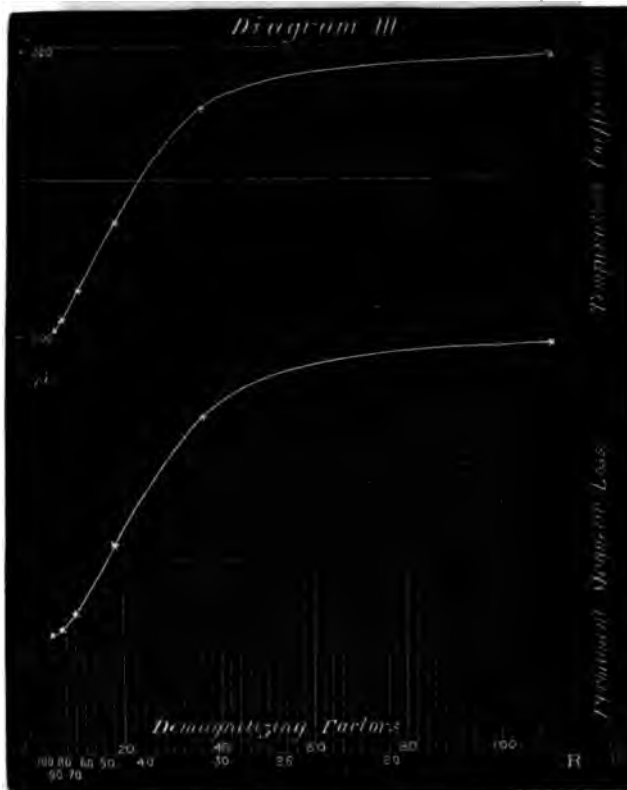
* Barus and Stronhal, 'Bulletin U.S. Geol. Survey,' No. 14, 1885.
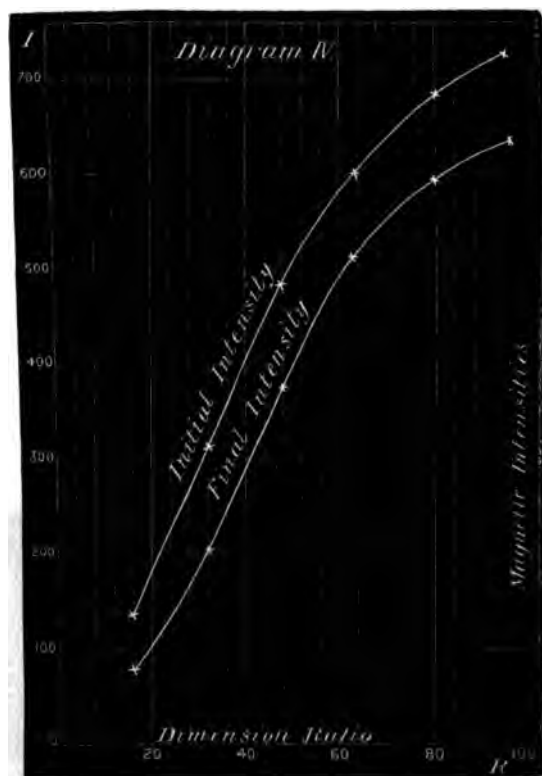
irons and steels, and is particularly small in hardened cast iron. Certain hardened nickel steels have very small negative coefficients.

2. The discovery of negative coefficients in music wires.

3. Change of the sign of the coefficient by alteration of (*a*) temper and (*b*) dimension ratio, and hence methods of obtaining zero coefficients.

4. Some relations between the dimension ratio and self-demagnetising factor, temperature coefficient, and permanent loss of magnetism after alternate heatings and coolings.

An important consideration in any practical application to magnetic instruments of magnets with zero coefficients is the constancy of the zero state.

It is not yet possible to speak precisely on this point, but two wires which had been prepared by adjustment of temper to have zero coefficients in June, 1896, and since then had been lying on a shelf, and in the vicinity of other magnets, when tested nine months later, had not altered so much as to have a coefficient of practical

consequence. The intensity had diminished, however, by nearly 25 per cent.

Similarly the magnet which had been given a negligible coefficient by cutting the length of the wire to 8 cm., as cited above (p. 219), after being boiled at intervals for four hours, was found five months later to have changed so little that its coefficient might still be considered negligible.

Further experiments, however, upon this question and some others arising out of this investigation are now in progress.

"The Electric Conductivity of Nitric Acid." By V. H. VELEY, M.A., F.R.S., and J. J. MANLEY, Daubeny Curator of the Magdalen College Laboratory, Oxford. Received November 1,—Read December 9, 1897.

(Abstract.)

In this paper an account is given of determinations of the electric conductivity of nitric acid of percentage concentrations varying from 1·3 to 99·97, purified, so far as possible, from reduction products of the acid, as also from sulphuric and the halogen acids, with which it is likely to be contaminated from its process of manufacture. In the preliminary experiments it was observed that the results might be vitiated by (i) a trace of nitrous acid either directly added or produced by decomposition due to exposure to sunlight, and (ii) imperfect insulation of the electrolytic cell caused by metallic clamps, a point which seems to have been neglected by previous observers.

The methods adopted for the purification of the water and nitric acid, as also for the detection and estimation of the impurities, are described in full. The greatest quantity of nitrous acid, sulphuric acid, and the halogen acids found in any sample used were 0·75, 4·3, and 3·8 parts per million respectively.

The thermometers, resistance coils, and other instruments used were compared with certain standards and corrected accordingly; the burettes and electrolytic cells were calibrated by one or more methods, and the mean of the values accepted.

The method adopted for the determinations was in outline that originally described by Kohlrausch, but modified so as to overcome certain difficulties experienced. A particular form of bridge was constructed, in which the wire was an air line, and a special form of slider adopted to tap without sagging the wire, so arranged that it could be moved by the observer from the extremity of the bridge, and thus all thermo-currents due to his proximity were avoided.

A rapidly revolving commutator was substituted for the usual induction coil, as the latter was found to be unsatisfactory owing to the susceptibility of nitric acid to polarisation.

Various forms of electrolytic cells were used according to the concentration of the acid and the temperature of the observations; these were provided with movable electrodes, so as to throw into circuit different lengths of acid.

A special form of apparatus was devised to prepare nitric acid of 99·88 per cent., and another form to obtain acid of 99·97 per cent. from the latter. As a considerable quantity of this practically anhydrous acid was obtained, its chemical and certain physical properties were examined. It has no action on (i) copper, (ii) silver, (iii) cadmium, and (iv) mercury, all of high degree of purity, and (v) commercial magnesium, at ordinary temperatures; purified iron and commercial granulated tin were unaffected by the acid, even when boiling. Purified zinc was slightly acted upon, but sodium immediately caught fire. The acid has no action whatever on calcium carbonate at ordinary temperatures or the boiling point. Flowers of sulphur and iron pyrites dissolve quickly and completely in the gently warmed acid. The following results were obtained for the density of the 99·97 per cent. acid, corrected for weighings *in vacuo* :—

Density 4/4 = 1·54212; 14·2/4 = 1·52234; 24·2/4 = 1·50394,

the mean values of two concordant observations.

As a further check upon the measurements obtained by the Kohlrausch method, certain other measurements were made by Carey Foster's method for the comparison of resistances, and the results obtained were found to be concordant within the limits of experimental error. In a series of tables the values are given for thirty-two samples of acid of the specific resistance in true ohms at temperatures of 0°, 15°, and 30°, the temperature coefficients $a 10^6$ and $\beta 10^8$ deduced from the equation $R_t = R_0(1 + at - \beta t^2)$, as also for $K_0 \times 10^8$, $K_{15} \times 10^8$, and $K_{30} \times 10^8$ (the conductivity of mercury at 0 being taken as unity, and its specific resistance as 94·07 microhms per 1 c.c.).

It is shown that the specific resistance decreases for percentage concentrations from 1·30 to 30, at first more, then less rapidly (thus confirming the previous observations of Kohlrausch); from this point the resistance increases slowly up to 76 per cent., thence more rapidly until a maximum is reached at 96·12 per cent., when a sudden reversal takes place.

Further, whereas nitric acid behaves as other electrolytes in possessing a positive temperature coefficient of conductivity for percentage concentrations from 1·3 to 96·12, yet from this point up to

99·97 per cent. it behaves as a metallic conductor in possessing a negative temperature coefficient.

Similar phenomena have been observed by Arrhenius in the cases of moderately dilute solutions of hypophosphorous and phosphoric acids, and explained by him by means of the ionic dissociation hypothesis. It is pointed out that nitric acid of 96—99·97 per cent. would *ex hypothesi* contain few, if any, free ions, and therefore the theory would lead to a totally opposite conclusion.

The results of the experiments are also discussed in relation to the hydrate theory of solution, and the illustrative curves in which the percentages of acid are taken as abscissæ and the resistances or conductivities in mercury units show points of discontinuity markedly at percentages corresponding approximately to the composition required for the hydrates $HNO_3,2H_2O$, HNO_3,H_2O, $2HNO_3,H_2O$ ($= H_4N_2O_7$), and less markedly for the hydrate $HNO_3,10H_2O$. Further, if the values of $\alpha \times 10^4$ and $\beta \times 10^6$ are referred to molecular proportions of water, the minima values of the former and the maxima of the latter occur in the cases of 3·07, 1·84, 0·99, and 0·55 molecular proportions or very approximately $HNO_3,3H_2O$, $HNO_3,2H_2O$, HNO_3,H_2O, and $2HNO_3,H_2O$. Further evidence is thus added by an independent method to that already accumulated as to the existence of definite combination of nitric acid with water. Finally, it is pointed out that if a curve is plotted out in which the molecular proportions of water are taken as abscissæ and the values for $\alpha 10^4$ as ordinates, there are ascending and descending branches, meeting at the points corresponding to the formation of the respective hydrates; the phenomena are compared with those observed by Bakhuis-Roozeboom for the solubility curves of hydrates of ferric chloride and by Le Châtelier, as also by Heycock and Neville for the freezing point of alloys.

"On the Refractivities of Air, Oxygen, Nitrogen, Argon, Hydrogen, and Helium." By Professor WILLIAM RAMSAY, Ph.D., LL.D., Sc.D., F.R.S., and MORRIS W. TRAVERS, B.Sc. Received November 18,—Read December 9, 1897.

In the course of a research on the nature of helium many measurements of its refractivity referred to that of air as unity were made by means of an apparatus similar to that described by Lord Rayleigh.[*] Inasmuch as the refractivity of helium is very small it was not found convenient to measure its value directly against air; hence it was compared with hydrogen, and hydrogen was compared with air.

[*] 'Proceedings,' vol. 59, p. 203.

And as a check on these measurements, the hydrogen was compared with oxygen and subsequently with nitrogen free from argon. It was noticed, after some of these experiments had been made, that the refractivity of air could not be accurately calculated from the given data for oxygen, nitrogen, and argon; and it appeared therefore worth while to examine more minutely the refractivity of these gases for white light, and to see whether any error could be detected in previous measurements. Moreover, as physicists perhaps do not always devote sufficient care to the chemical purity of their materials, an additional reason was furnished for the inquiry.

Apparatus.—It will be seen, on consulting Lord Rayleigh's paper, that the refractivity is measured in the following manner:—Light from a paraffin lamp passes through a fine slit, cut with a razor in tin-foil pasted on glass. The beam is made parallel by passage through an achromatic plano-convex lens of about 1 foot focal length. It then divides; the upper portion passes through air, and, after extraneous light is cut off by passage through two wide slits, it is brought to a focus by a lens similar to the first, and the bands produced are viewed by a cylindrical lens of very short focus. The lower portion of the beam traverses two tubes, 9 inches long and one-quarter of an inch in diameter, placed close together, and closed at each end with plates of optically worked glass. Each of these tubes contains one of the gases to be examined; and each is connected with a manometer and a movable reservoir; so that, on raising or lowering the reservoir, the pressure of the gases can be so adjusted that the interference-bands formed in the lower half of the field can be accurately brought into line with the stationary bands in the upper half. Readings of pressure are taken on both manometers at pressures not differing greatly from that of the atmosphere; then, on lowering the reservoirs, readings on both manometers are taken at lower pressures, the bands being again made to coincide in position with the upper fiducial bands. The ratio of the refractivities is inversely as the differences of pressure in the two gases. The influence of temperature does not appear, for the tubes of the manometer lie side by side, and may be regarded as equally affected by variations of temperature.

The accuracy of this method varies with the value of the refractivity of the gas. For, if the gas has a low refractivity, then a great difference of pressure produces the passage of fewer bands across the field than if it has a high one; and, as the accuracy of reading may safely be taken as the twenty-fifth of a band, and as between thirty and forty bands passed the field with such gases as oxygen, nitrogen, and argon, the error may be taken in such cases as from 1 in 750 to 1 in 1000.

The tubes containing the gases to be examined were connected

with a Töpler's pump; and before admission of gas each tube was pumped empty, so that in an attached Plücker's tube there was brilliant phosphorescence. The tubes were then washed out with the gases to be admitted, the apparatus again evacuated, and the final quantity of gas allowed to enter by a contrivance a description of which is to be found in the 'Trans. Chem. Soc.,' vol. 67, p. 686.

Purity of the Gases. Hydrogen.—The hydrogen was made by warming a tube containing palladium-hydrogen which had been prepared by admitting hydrogen made from pure zinc and sulphuric acid into contact with spongy palladium. The tube was pumped empty in the cold, and then gently warmed; it was again allowed to cool and again pumped empty. The hydrogen was then collected, passing slowly through a tube, filled with phosphoric anhydride, into the experimental tube.

Oxygen.—The oxygen was prepared by heating a small tube containing potassium permanganate; a large quantity of gas was allowed to escape, and a portion was collected finally which served for the experiments.

Nitrogen.—The nitrogen was prepared from a mixture of ammonium chloride and sodium nitrite, to which a little copper sulphate had been added. The apparatus was exhausted before admission of either of the solutions, and before allowing the solutions to enter they were boiled, and the flasks corked while boiling. The gas was passed over red-hot copper; the ammonia liberated by the alkalinity of the nitrite thus reacted with any oxides of nitrogen possibly present to form water. The gas was collected, after rejection of a considerable portion, in a tube containing oil of vitriol; it was then transferred to a fresh tube, treated with a very strong solution of caustic potash, and finally admitted to the apparatus.

Air.—The air was left standing for some hours in a tube containing sticks of caustic potash, and was then admitted to the apparatus through a tube of phosphoric anhydride.

Experimental Data.—Each gas was compared with air and with the other two. Air is in each case taken as unity.

Hydrogen.—Hydrogen/air	0·4730	Mean ..	0·4733
	0·4737		
Hydrogen/oxygen ...	0·5125	„ ..	0·5125
	0·5125		
Hydrogen/nitrogen ..	0·4654	„ ..	0·4654
	0·4654		
Oxygen.—Oxygen/air	0·9237	·	
	0·9262	„ ..	0·9243
	0·9230		

Oxygen.—Oxygen/hydrogen ... 1·9512   Mean .. 1·9512
                               1·9512

          Oxygen/nitrogen.... 0·9090
                               0·9122   ,, .. 0·9105
                               0·9103

Nitrogen.—Nitrogen/air........ 1·0153
                                 1·0171   „ .. 1·0166
                                 1·0174

          Nitrogen/hydrogen.. 2·1487
                               2·1487   „ .. 2·1487

          Nitrogen/oxygen.... 1·1001
                               1·0962   „ .. 1·0983
                               1·0986

. To these numbers those for argon must be added. The gas was prepared in the usual manner from air; and before admitting it into the experimental tube it was sparked with oxygen in presence of caustic soda for two days. The oxygen was removed with phosphorus, and the argon, on its way into the experimental tube, passed over phosphorus pentoxide; a Plücker's tube was sealed to the tube through which it entered, so that its spectrum might be observed. It contained no visible trace of either hydrogen or nitrogen.

Argon.—Argon/air .... 0·9596
                                 0·9596   Mean .. 0·9596
                                 0·9598

          Argon/oxygen......... 1·0350
                                 1·0348   „ .. 1·0362
                                 1·0381

          Argon/nitrogen .... 0·9412
                                  0·9419   ,, . .. 0·9416

Placing air in each case equal to unity, and calculating the refractivities of the other gases, we obtain the following table :—

Refractivities of Gases, Air equal to Unity.

	Directly compared.	Through			
		Oxygen.	Nitrogen.	Hydrogen.	Argon.
Hydrogen	0·4733	0·4737	0·4727	—	—
Oxygen	0·9243	—	0·9247	0·9237	0·9261
Nitrogen	1·0163	1·0155	—	1·0170	1·0191
Argon........	0·9596	0·9577	0·9572	—	—
Carbon dioxide	—	1·5316	—	—	—

It will be seen on inspecting the above table that the numbers obtained indirectly are in close agreement with those obtained by direct comparison with air.

Taking the value found directly by Mascart for D*, viz. $(n-1)_D = 0.0002923$, the value found by him for nitrogen (atmospheric) was 0.0002972, giving for nitrogen, on the basis air equal to unity, the number 1.0178. Mascart did not determine the value for oxygen, but calculated it from the above data and the known composition of air. Nor did Lorenz determine the value for nitrogen; but taking his own value for oxygen, viz. $(n-1)_D = 0.000272$, and for air $(n-1)_D = 0.000291$, he deduced it, as Mascart had done for oxygen. So that we have no determination of the three constants, or their comparison, by any one observer since Dulong in 1826. It has been tacitly assumed that the refractive index for a mixture of gases is that of those of their consituents, taken in the proportion in which they occur. We have in our hands a means of verifying this assumption, which is well known not to hold for compound gases, nor for mixtures of liquids, even though change of density be taken into consideration.

Dulong† gives very careful accounts of the methods he used in preparing the samples of gas that he employed. Oxygen, to which he ascribed the refractivity 0.924, was obtained by heating potassium chlorate. His result is identical with ours. Nitrogen was prepared from air by absorbing the oxygen with phosphorus, first at a high temperature and then in the cold. It was then washed with a solution of chlorine, and afterwards with potash. It is difficult to see what object was to be gained by washing with chlorine water, unless it was the removal of hydrogen. The number he obtained was 1.02, somewhat higher than that which we have found. Dulong also determined the refractivity of air, and allowing for that of the small percentage of carbon dioxide, it is precisely the mean of that of its constituents, taken in the proportion in which they are present.

Returning to the results of Mascart and Lorenz, we have for the D lines :—

	Air.	Nitrogen.	Oxygen.
Mascart	1	1·0178	—
Lorenz........	1	—	0·9347

From these data of Mascart and Lorenz it is possible to calculate the refractivity of air :—

$$(1.0178 \times 79.1) + (0.9347 \times 20.9) = 100.15.$$

There is reason to doubt the purity of Lorenz's oxygen. He heated

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* The dispersions for these gases are so small as not to affect the ratios of these numbers ('Compt. Rend.,' 1874, vol. 78, p. 621).

† 'Ann. Chim. Phys.,' vol. 31, p. 176, 1826.

mercuric oxide, of which he does not give the method of preparation ;
it may have contained oxides of nitrogen; and for some reason, not
explained, he passed the gas through a vacuous porcelain tube, pre-
sumably red-hot, which, as recent experiments of Messrs. Bone and
Jerdan have shown,* is not impervious to furnace gases. Dulong,
on the other hand, who, as already remarked, prepared his oxygen
from chlorate, obtained the number 0·924 for white light, coincident
with our determinations.

The refractive index of air, calculated from our determinations,
viz.,

Oxygen............... 0·9243
Nitrogen .... 1·0163
Argon............... 0·9596

and the densities of the constituent gases,† gives the following
numbers :—

$$(1·0163 \times 78·15) + (0·9243 \times 20·91) + (0·9596 \times 0·94) = 99·653.$$

Observers sometimes find the percentage of oxygen in air to be
about 20·98, or even 21·0. This would hardly affect the result; with
20·96 per cent. of oxygen the calculated refractivity is 99·647, instead
of 99·653.

There can be no doubt as to the refractivity of oxygen from our
ratios, as well as from Dulong's determinations. The question is as
regards nitrogen. It would require the refractivity of nitrogen to be
1·208, a number greatly above any of our values, in order that the
sum of the refractivities of oxygen, nitrogen, and argon should equal
100. The presence of argon would also make an almost inappreciable
difference. Taking Mascart's determination of the refractivity of
atmospheric nitrogen to be correct, that of pure nitrogen would be
1·0181, instead of 1·0178. And an error in the refractivity of argon
would also not affect the result, inasmuch as the total amount of
argon is so small.

We are thus driven to conclude that the refractivity of the
mixture, air, is somewhat less than that of the sum of the refrac-
tivities of its constituents, taken in the proportion in which they
occur.

It appeared advisable to try other mixtures; and a mixture of
hydrogen and helium was first selected, because these are both very
"perfect" gases, inasmuch as their critical points lie very low. It
was to be expected that if a difference between calculated and found
values should exist, it should be of the inverse character to that of a

* 'Chem. Soc. Trans.,' 1897, p. 42.
† Argon, 'Phil. Trans.,' A, 1895, p. 202, foot-note.

mixture of oxygen and nitrogen, for they are two somewhat " imperfect " gases. The result has borne out this idea.

A mixture was made of 20·60 c.c. of hydrogen and of 20·12 c.c. of helium free from argon, and of the density 1·960; and with the refractivity of the mixture those of hydrogen and helium were compared. Taking the refractivity of the mixture as unity, the following ratios were found :—

Hydrogen/mixture ...... 1·5977   Mean.. 1·5967.
                        1·5957

Helium/mixture........ 0·4513   „ .. 0·4495.
                       0·4478

The calculated values are—

$$\frac{(·04495 \times 20·12)}{40·72} = 22·21.$$

$$\frac{(1·5967 \times 20·60)}{40·72} = \frac{80·87}{102·99}.$$

Here the calculated value of the refractivity of the mixture is 3 per cent. higher than the found value, while with air the calculated value is 0·35 per cent. too low.

A third experiment was made, in which the " artificial air " was a mixture of 19·13 c.c. of carbon dioxide with 19·29 c.c. of oxygen, both gases supposed to be at 0° and 760 mm. Again, taking the refractivity of the mixture as unity we found the following ratios :—

Carbon dioxide/mixture .... 1·2450
Oxygen/mixture .... 0·7525

The calculated values are :—

$$\frac{(1·2450 \times 19·13)}{38·42} = 61·99.$$

$$\frac{(0·7525 \times 19·29)}{38·42} = \frac{37·78}{99·77}.$$

Here, as with air, the total refractivity found is less than that calculated. It is true the difference is not great, but we are persuaded that it is real, for it considerably exceeds the error of our several determinations.

The case is not bettered if Lorentz and Lorenz's formula be substituted for Gladstone and Dale's. Using their formula, n^2-1/n^2+2, the calculated result is 99·72 per cent. of that found for air.

The coefficient of compressibility of hydrogen is too small, while that of other gases, such as oxygen and nitrogen, is too great. The

effect of mixing equal volumes of hydrogen and helium, each of which has too large a coefficient of elasticity, is to cause each to occupy twice the volume that they previously occupied, and to halve approximately the pressure for each.   The pressure is therefore lower than it would be for an absolutely ideal gas, for each gas, hydrogen and helium.   The sum of these pressures will accordingly be too low, or transposing, the sum of the volumes will be too great. The opposite argument holds for air.

Now, in considering volumes we deal not merely with the co-volume, *i.e.*, the space occupied by the molecules, but also with the interstitial space inhabited by the molecules.   But the refractive power, if Clausius's deduction from the formula of Lorenz and Lorentz is correct, is a function of the dielectric constant, and hence of the co-volumes of the gases.   And here the discrepancy is more easily detected than by any determination of density.   It must therefore be concluded that gases are not, as postulated by Dalton, indifferent to one another's presence, but that they modify one another's properties in the same manner as do liquids, though to a different extent.   This mutual action at high pressures and small volumes modifies even the volume relations, as recently shown by Dr. Kuenen.   And it must persist at low pressures and large volumes, though it may not always be possible to make measurements of pressure and volume accurate enough to lead to its detection.   The refractivity, however, seems to be a means delicate enough to be used for this purpose.

"On the Openings in the Wall of the Body-cavity of Verte-
    brates."   By EDWARD J. BLES, B.Sc. (Lond.), King's
    College, Cambridge.   Communicated by Dr. HANS GADOW,
    F.R.S.   Received June 16,—Read June 17, 1897.

In the review of the vertebrates held in the following pages, I have put together as many facts as I could ascertain on the distribution of abdominal pores in the various groups, and side by side with this evidence I have arranged the available facts recorded by others, and observed by myself, on the distribution of nephrostomes and other openings on the wall of the abdominal cavity.

By so doing, the physiological meaning of the abdominal pores has, I believe, been elucidated through the evidence of a correlation, speaking generally, of an alternative character, between these two sets of organs.   It will further appear that in most of the higher vertebrates —where abdominal pores do not occur and nephrostomes disappear early in development or lose their original connection with the renal ducts— the body-cavity has taken upon itself a different functional character.   Instead of acting as auxiliary to the excretory organs, it takes part in the internal work of the circulatory lymphatic system.

The greater part of the information on the openings from the periviceral cavity to the exterior in the Elasmobranchii is contained in two papers; one by Semper,[*] on the urogenital system of Plagiostomes, the other, by Bridge, on " Pori Abdominales of Vertebrata."[†] Semper describes the persistence in certain Elasmobranchs of a number of open segmental funnels on the peritoneal epithelium leading into the Malpighian bodies of the mesonephros. Such funnels occur in all Elasmobranch embryos, but usually close during development. Semper gives lists of species with and without nephrostomes when adult, and shows that their presence cannot be correlated with the presence or absence of other organs, among which he did not, however, refer to the abdominal pores. Bridge was the first to examine a number of Elasmobranchs expressly to determine the distribution of abdominal pores amongst the species of these fishes. He states that it was " not clear that the presence or absence of the pores can be correlated with structural variations in other organs." I was led to compare Semper's and Bridge's accounts of the distribution of nephrostomes and abdominal pores, and it at once became evident that their presence in Elasmobranchs was, to a certain extent, reciprocal. A few discrepancies which appeared have been investigated, and my results, although they agree in the main with Bridge's, differ from his in one or two important cases. A detailed discussion of these cases will appear elsewhere.

The species which have come under my own observation are :—
Carcharias acutus, Rüppel, *C. glaucus,* L., *Galeus canis,* Bonap., *Zyyœna malleus,* Risso, *Mustelus vulgaris,* M. and H., *M. lœvis,* Risso, *Hexanchus griseus,* Gm., *Heptanchus cinereus,* Gm., *Scyllium canicula,* L., *S. stellare,* L., *Pristiurus melanostomus,* Bonap., *Oestracion philippi,* Lacép., *Spinax niger,* Bonap., *Scymnus lichia,* Cuv., *Centrophorus granulosus,* Bl. Schn., *Rhina squatina,* L., *Pristiophorus cirratus,* Lath., *Pristis zysron,* Blkr., *Rhinchobatus djeddensis,* Forsk., *Rhinobatus granulatus,* Cuv., *Torpedo narce,* Risso, *Narcine brasiliensis,* Olf., *Raja clavata,* L., *R. maculata,* L., *Myliobatis maculata,* Gray, and *Myl. sp.*

Table I contains all the species for which there are data respecting both nephrostomes and abdominal pores. It includes all the species investigated by Semper, excepting *Lamna glauca,* M. and H., and *Temera hardwickii,* Gray. In these two species the nephrostomes close, and it may be expected that they will eventually be found to possess abdominal pores.

Table II is so arranged that the species with nephrostomes are

[*] C. Semper, "Das Urogenitalsystem der Plagiostomen und seine Bedeutung für das der übrigen Wirbelthiere," ' Arb. Zool.-zoot. Inst. Würzburg,' vol. 2 (1875), pp. 195—509.

[†] ' Journ. Anat. and Phys.,' vol. 14 (1879), pp. 81—100.

placed together under A, and the species without them together under B. The species under B all possess abdominal pores. Under A, on the other hand, there is no such uniformity as regards the pores. We have here a fairly complete series of species; beginning with forms without abdominal pores like Cestracion, passing to forms like *Scy. stellare*, which acquire pores late in life and may occasionally fail to do so; we then come to the *Scy. canicula* group, where pores are found at the stage of sexual maturity, but where they may be acquired still later or sometimes not at all; and, lastly, there is a fourth group, that of *Acanthias vulgaris*, where the pores appear at an early age, towards the end of embryonic life, and seem to be invariably present. These four groups have one character in common: the nephrostomes remain open in the adult. In this they differ from the species under B, which close the nephrostomes early in development, and then, like Group 4 of Series A, and likewise at an early age (Carcharias during fœtal life), open abdominal pores.

To some of the species in Table I no place in Table II can be assigned until more specimens of different ages have been examined.

It is sufficiently obvious from the list of species in Series B of Table II that abdominal pores are distributed without reference to oviparity or viviparity.

Table I.

Elasmobranchii.

	Segmental tubes.		Abdominal pores.	
	Open.	Closed.	Present.	Absent.
Sub.-Order SELACHOIDEI.				
Fam. 1. Carchariidæ.				
a. Carchariina.				
Carcharius glaucus, L.		—	+	
Galeus canis, Bonap.		—	+	
b. Zygænina.				
Zygæna malleus, Risso, ♂				
juv.		—	+	
c. Mustelina.				
Mustelus vulgaris, M and				
H.		—	+	
M. lævis, Risso, ♀		—	+	
Triacis semifasciata, Girard		—	+	
Fam. 2. Lamnidæ.				
Lamna cornubica, Gm.		—	+	
(Fam. 3. Rhinodontidæ.)				
Fam. 4. Notidanidæ.				
Hexanchus griseus, Gm., ♀ juv.	+		+	
Heptanchus cinereus, Gm., ♀ . .	+		(or +)	—

Table I—*continued.*

	Segmental tubes.		Abdominal pores.	
	Open.	Closed.	Present.	Absent.
Sub-Order SELACHOIDEI—*cont.*				
Fam. 5. Scylliidæ.				
Scyllium canicula, L., ♂, 7 cases (E. J. B.)............	+		+ (5 cases)	− (2 cases)
Ditto, ♂ (Bridge)	+			−
Ditto, ♂ (Marshall and Hurst)	+		+	
Ditto, ♀, 12 cases (E. J. B.) ..	+		+ (6 cases)	− (6 cases)
Ditto, ♀ (Bridge)	+			−
Ditto, ♀ (Marshall and Hurst)	+		+	
Ditto, ♀ juv., (ditto)	+			−
Scyllium stellare, L.	+		(or +)	−
Pristiurus melanostomus, Bonap.	+		(or +)	−
Fam. 6. Cestraciontidæ.				
Cestracion philippi, Lacép., ♀ .	+			−
Fam. 7. Spinacidæ.				
Centrina salviani, Risso, ♀ ..	? +		+	
Centrophorus granulosus, Bl. Schn.	+		+	
Ditto, ♂ juv.	+			−
Scymnus lichia, Cuv.	+		+	
Acanthias vulgaris, Risso	+		+	
Ditto, 12″ fœtus (Bridge)	+			−
Spinax niger, Bonap., ♂	+		+	
Ditto, ♀.....................	+		+	(or −)
Fam. 8. Rhinidæ.				
Rhina squatina, L.	+			−
(Fam. 9. Pristiophoridæ.)				
Sub-Order BATOIDEI.				
(Fam. 1. Pristidæ.)				
Fam. 2. Rhinobatidæ.				
Rhinchobatus djeddensis, Forsk., ♀		−	+	
Rhinobatus granulatus, Cuv. ..		−	+	
Fam. 3. Torpedinidæ.				
Torpedo narce, Risso		−	+	
„ *marmorata*, Risso		−	+	
Hypnos subnigrum, Dum., ♂ ..		−	+	
Narcine brasiliensis, Olf.		−	+	
Fam. 4. Rajidæ.				
Raja clavata, L.		−	+	
„ *maculata*, Montag., 10 cases (E. J. B.)		−	+	
„ *punctata*, Risso		−.	+	
„ *miraletus*, L.		−	+	
„ *batis*, L.		−	+	
„ *marginata*, Lacép.		−	+	
„ *blanda*, Holt and Calderwood		−	+	
Fam. 5. Trygonidæ.				
Trygon brucco, Bonap.........		−	+	
„ *pastinaca*, L.		−	+	
Fam. 6. Myliobatidæ.				
Myliobatis maculata, Gray, ♀		−	+	
„ *sp.*, ♀ juv.		−	+	

Table II.

A. Elasmobranchs with nephrostomes when adult.
  (1) Abdominal pores absent.
      Cestraciontidæ. *Cestracion philippi*, Lacép.
      Rhinidæ. *Rhina squatina*, L.
  (2) Abdominal pores absent until full-grown. (Whether they are then constantly present is not known.)
      Scylliidæ. *Scyllium stellare*, L.
              Pristiurus melanostomus, Bonap.
  (3) Abdominal pores appear late (when sexually mature) and may be absent.
      Scylliidæ. *Scyllium canicula*, L.
      Spinacidæ. *Spinax niger*, Bonap.
  (4) Abdominal pores appear early and are constantly present.
      Spinacidæ. *Acanthias vulgaris*, Risso.
              Symnus lichia, Cuv.
B. Elasmobranchs without nephrostomes when adult.
  (5) Abdominal pores always present.
      SELACHOIDEI. Carchariidæ. *Carcharias glaucus*, L. *Galeus canis*, Bonap., *Zygæna malleus*, Risso, *Mustelus vulgaris*, M. and H., *M. lævis*, Risso, *Triacis semifasciata*, Girard.
      Lamnidæ. *Lamna cornubica*, Gm.
      BATOIDEI. Rhinobatidæ. *Rhinchobatus djeddensis*, Forsk.
              Rhinobatus granulatus, Cuv.
      Torpedinidæ. *Torpedo narce*, Risso, *T. marmorata*, Risso, *Hypnos subnigrum*, Dum., *Narcine brasiliensis*, Olf., *Raja clavata*, L., *R. maculata*, Montag., *R. blanda*, Holt and Calderwood, *R. punctata*, Risso, *R. miraletus*, L., *R. batis*, L., *R. marginata*, Lacép.
      Trygonidæ. *Trygon brucco*, Bonap.
              T. pastinaca, L.
      Myliobatidæ. *Myliobatis maculata*, Gray,
              Myliobatis sp.

The most obvious of the facts brought into prominence by the above tables are the following :—

1. The Elasmobranch fishes when adult have a peritoneal cavity which in every known instance communicates with the exterior.

2. This communication is established in several ways :—
   (a) Through nephrostomes and urinary canals ;
   (b) More directly, through abdominal pores ; but
   (c) Both communications may exist together in the same individual.

3. Disregarding for the moment the eight species which, roughly speaking, possess both sets of openings, there remain twenty-eight species in which a correlation of an alternative nature

exists between the renal communications on the one hand and the abdominal pores on the other. There are four species with persistent nephrostomes and practically without abdominal pores, and twenty-four species with abdominal pores which lose the nephrostomes very early in development.

4. The Batoidei and the Carchariidæ are characterised by the absence in the adult of open nephrostomes and the presence of abdominal pores.

In the above statements no account has been taken of the third channel of communication present in all female Elasmobranchs, that practicable through the oviduct after the disappearance of the hymen.

The relations thus brought to light afford a fairly complete account of the distribution of both nephrostomes and abdominal pores amongst adult Elasmobranchs. The total number of species (thirty-six) from which these conclusions have been drawn is somewhat small when compared with the number of species on record in this group, viz., more than 300. The families of the group are, however, all represented, with the exception of the Rhinodontidæ, the Pristiophoridæ, and the Pristidæ, in which nothing is known of the nephrostomes; but I found abdominal pores in a species from each of the Selachian and Batoid families of Sawfishes. It follows then that with the two species referred to and six additional species from various families of Selachians known to possess abdominal pores,* there are forty-four species in which the abdominal cavity is known to communicate with the exterior, and not one in which this cavity is shut off from such communication. So that we may extend the generalisations arrived at from the discussion of the material here brought together to the whole of the Elasmobranchii. It may hence be concluded that the body cavity of adult Elasmobranchs is never completely enclosed by the peritoneum; it always communicates with the exterior, the communication being effected in various ways, as shown above.

The distribution of species with nephrostomes and of species without them in the Elasmobranchii is clearly shown in Table I. The scheme of classification adopted is from Dr. Günther's ' Catalogue of Fishes in the British Museum' (published in 1870).

A glance at Table I shows that the Carchariidæ all close the segmental tubes, three species of Scyliidæ retain them, four, probably five, species of Spinacidæ do the same; also two species of

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* These eight species are :—*Pristis zysron*, Blkr., *Pristiophorus cirratus*, Lath., *Echinorhinus spinosus*, Gm., *Læmargus borealis*, Scoresb., *Chlamydoselachus anguineus*, Garm., *Zygæna tudes*, Cuv., *Carcharias melanopterus*, Q. and G., and *Carcharias acutus*, Rüppel.

Notidanidæ do so; while all the Batoidei lose the peritoneal nephri-dial openings.   Thus the classification here employed is in harmony with the distribution of nephrostomes in adult Elasmobranchs.

Table III.—In this table I have placed, side by side, the data respecting the distribution of nephrostomes and of abdominal pores in all the Vertebrates not yet dealt with, excluding the Cyclostomes. The latter are omitted on account of the great probability that the so-called abdominal pores of Cyclostomes are genital ducts, morpho-logically as well as functionally, and hence quite distinct from true abdominal pores, which are never used as genital ducts.

The fishes in which the ova when mature fall into the general body cavity usually have abdominal pores; this applies to certain Elas-mobranchs, the Holocephala, most Ganoids, some Dipnoi, and to some Salmonidæ.   But there are exceptions, as we have seen, amongst the Elasmobranchs; some Salmonidæ have no pores, and other Teleos-teans with the primitive form of ovary have none, *e.g.*, Murænidæ, Galaxidæ, &c.   Then, on the other hand, there are fishes with tubu-lar closed ovaries forming a continuous tube with the oviducts and with a lumen quite distinct from the body-cavity, as it is in the majority of Teleosteans, yet the body-cavity is, nevertheless, open to the exterior through abdominal pores.   *Lepidosteus osseus* is in this condition, and, amongst Teleosteans, the Mormyridæ.   It follows that there is no correlation between the condition of the ovary and the occurrence of abdominal pores.

It is noteworthy that in the Amphibia alone, amongst the Verte-brates ranking higher than Elasmobranchs, do the nephrostomes persist in the adult, and that the abdominal pores are entirely absent. This is the more striking, since the abdominal pores reappear in Reptiles, in the absence of nephrostomes.

Equally important, from the present point of view, is the change of function of the nephrostomes within the group Amphibia.   In the Gymnophiona and Urodela the original excretory function is served; but in Anura, where the lymphatic system is more highly developed, the body-cavity has become a lymph space, and the nephrostomes lead from it directly into the renal veins, not into the kidney tubules (Nussbaum).   The abdominal cavity of Anura is also, through the stomata, in close communication with the lymphatic system, and so, through the lymph-hearts, with the veins.   Hence the body-cavity of the lower Amphibia may be compared, as regards its relations to the kidneys, with the body-cavity of certain Elasmobranchs (Cestracion), while the relations of body-cavity and vascular system in the Anura are parallel to those obtaining in the higher Reptilia and in Mammals.

Table III.

	Segmental tubes.		Abdominal pores.	
	Open.	Closed.	Present.	Absent.
HOLOCEPHALA.				
Chimæra monstrosa		—	+	
GANOIDEI.				
Lepidosteus osseus	(? + into lumen of ovary) Balfour and Parker.	— in peritoneal cavity.	+ (Joh. Müller, Hyrtl).	—
Sturiones	—	— (*Acc. sturio*, Jungersen)	+ (Joh. Müller, Hyrtl).	
Polyodon folium			+ (A. Wagner, Hyrtl).	
Polypterus bichir			+ (Hyrtl).	
Calamoichthys calabaricus ..			+ (Bridge).	
Amia calva, adult.........			+ (Hyrtl, Bridge).	
„ „ larvæ, 16¼ mm.	+ (Jungersen).	— (Jungersen).		
DIPNOI.				
Protopterus annectens	—	— (Parker, Wiedersheim).	+ (Owen, Stannius).	— (Jungersen).
Ceratodus forsteri		(?)	+ (Günther).	
Lepidosiren paradoxa, Fitz.		(? — Ehlers).	— (Hyrtl).	
TELEOSTEI.				
Salmonidæ.				
Argentina silus, Risberg		—	+ (M. Weber).	
A. hebridica, Yar. spec.		—	+ „	
Coregonus lavaretus, L.		—	+ „	
Salmo trutta, L., ♂		—	+ „	
Coregonus oxyrhynchus, ♂ ♀.		—	+ „	
Salmo fario, L.		—	+ „	or — (M. Weber).
Salmo salar „		—	+ „	or — (G. Schneider).
„ „ juv.		—		— (M. Weber).
Osmerus eperlanus, L.		—		— „
Mallotus villosus, Cuv.		—		— „

Table III—*continued*.

	Segmental tubes.		Abdominal pores.	
	Open.	Closed.	Present.	Absent.
TELEOSTEI—*continued.*				
Mormyridæ.				
Mormyrus oxyrhynchus		—	+ (Hyrtl).	
,, *base*		—	+	
,, *cyprinoides*		—	+ ,,	
,, *elongatus*		—	+ ,,	
,, *dorsalis*		—	+ ,,	
,, *anguillaris*		—	+ ,,	
,, *zambacensis*		—	+ ,,	
,, *caschive*		—	+ ,,	
Gymnarchus niloticus		—	+ ,,	
All remaining Teleostei				—
(In ♀ Murænidæ, Galaxiidæ, Hyodontidæ, Bathythrissidæ, and Notopteridæ the body cavity opens to the exterior through the oviducts.)				
AMPHIBIA.				
Gymnophiona................	+			—
Urodela	+ (open into the renal veins).			—
Anura....................				—
REPTILIA.				
Hatteria..................		— (Wiedersheim).	(Vestiges, Gadow).	
Chelonia.		—		
Geoemyda grandis, ♂			+ (Anderson).	

Species			
Emys hamiltonii, ♂	—	+	
Trionyx ocellatus, ♂	—	+	
„ gangeticus, ♀	—	+	
Batagur thurgi, ♀	—	+	
Chitra indica, ♀	—	+	
Emys trijuga, ♀	—	+	
Testudo platynotus, ♀	—	+	— (Gadow).
Batagur lineatus, ♀	—	”	— (Fritsch).
Testudo microphyes, ♂	—	”	— (Gadow).
„ elephantopus, ♂	—	”	
„ græca, ♂	—	”	— (Lataste).
„ „ ♀	— (Bles).	+ (Gadow).	— (Mayer).
„ nemoralis, ♂	—	”	— (Bles).
Chelone midas, ♂	— (Wiedersheim).		
„ „ ♀	—		
Crocodilia.	—	+ (Bridge).	— (Bridge).
Crocodilus vulgaris	—	”	
Alligator sp., ♂ and ♀	—	+	
Crocodilus sp., ♂	—	+	
Lacertilia			
(Lacerta and Anguis fragilis have stomata on peritoneum.)	—		—
Ophidia. ⎱ (These groups have apparently not been examined for the presence of stomata.) Aves. ⎰	—		—
Mammalia (Stomata are present both on the pleural and abdominal serous membranes.)	—		—

Conclusions.

The first inference to be drawn from the facts as here presented is one bearing on the function of the abdominal pores. When some conclusion has been reached as to whether the pores are mere vestigial rudiments of no present physiological value, or whether they are of real functional value to those vertebrates in which they occur, then an attempt may be made to assign to them their morphological meaning.

The opinion held by Bridge as the result of his investigations on abdominal pores was, that, excepting in those cases where the genital products passed out through them, they had no definite function. It is now known that true abdominal pores do not serve as genital ducts; the so-called abdominal pores of Cyclostomes, Murænidæ, &c., are really genital ducts, and their homologies do not point to the abdominal pores of the Elasmobranchs, &c. The pores would, accordingly, be left without any particular function.

This conclusion is, I believe, contradicted by the facts connected with the relative distribution of nephrostomes on the one hand and abdominal pores on the other. In the Elasmobranchs three larger groups may be recognised. In one nephrostomes only occur, in another abdominal pores only, in the third nephrostomes are always present, while abdominal pores may or may not be added. Amongst the Holocephala abdominal pores alone exist in the adult *Chimæra monstrosa.* The Ganoids lose their nephrostomes, but all have abdominal pores. All that is clear about the Dipnoi points to the presence of abdominal pores in Ceratodus and in *Protopterus annectens,* nephrostomes being absent. The Teleostei with abdominal pores have no nephrostomes. The Amphibia Cæcilia and the Amphibia Urodela both have nephrostomes opening a passage from body-cavity to the exterior. In certain Chelonia and Crocodilia, which close their nephrostomes during embryonic life, abdominal pores are present. All the groups just mentioned have either nephrostomes or abdominal pores, or in some (intermediate) cases both together. In none are both openings absent, and it appears to be necessary for some passage to be open between the body-cavity and the exterior. This consideration gives, I believe, a clue to the function of the abdominal pores. As they alternate on the whole in their distribution with the nephrostomes, they undertake in all probability the duties of the absent nephrostomes, or when both are present together they act in mutual support of each other.

In what the function of the pores chiefly consists is demonstrated by a very interesting experiment recently performed by Guido Schneider.[*] He injected finely divided Indian ink mixed

* ' Anat. Anz.,' vol. 13, No. 15, p. 393.

with a little carmine into the body-cavity of specimens of *Squatina angelus* (*Rhina squatina*, L.). Two days later the animals were killed. Phagocytes laden with the colouring matter were found in the nephrostomial funnels and in spaces in the kidney into which these funnels led. Whether these spaces were continuous with the lumen of the renal tubules could not be determined. But in addition to this means of clearing the foreign matter away from the peritoneal cavity, another method, as I take it, appears to have been resorted to. I have satisfied myself that in the normal *R. squatina* there are no abdominal pores, but there are cloacal pits with exceedingly thin walls towards the body-cavity. From Schneider's description, I conclude that these walls were broken through in his more or less pathological specimens, forming ruptures on each side of the cloacal slit. They appeared as black pits pigmented with Indian-ink-laden leucocytes, wandering, as he explains, into the tissue round the pore. The pigmented leucocytes were found only in these two places, and the pores had opened, no doubt, I consider, in consequence of pressure from within, for the spaces in the kidneys are described as being choked with phagocytes. What appears to have happened is, that the nephrostomes having been stopped up with an accumulation of phagocytes, the pores broke open, and the remaining phagocytes were there expelled, some of them, however, passing into the exposed connective tissue round the edge of the rupture. Another previous experiment by Schneider gives further evidence on the subject.* He made a copious injection of carmine suspended in sterilised salt solution into the body-cavity of *Salmo fario*. His specimens may have been immature; in any case, unlike some of Weber's, they had no abdominal pores. But potentially, I am inclined to think, they were present, as the phagocytes which had engulphed the injected pigment collected at the posterior end of the body-cavity, where the abdominal wall on each side of the anus is very thin. Some of the phagocytes wandered into this thin wall, and in one case they passed right through it and formed a mass of pus on the external surface. This gives, I take it, a very plain hint of what would have happened if the abdominal pore had been present. There would have been no obstacle to the excretion from the peritoneal cavity of the foreign matter and products of inflammation.

The abdominal pores are then excretory ducts, and (should occasion arise) would, as Günther suggested, aid in removing stray ova and semen from the body-cavity as a part of their work, but by no means as the whole of it. And if this view is correct, the body cavity of fishes and of the lower Amphibia is to a great extent an excretory organ, as it certainly is in the early stages of the

* 'Mém. Ac. Imp. Sci.,' St. Pétersbourg, [8], 1895, vol. 2, No. 2.

development of almost all Vertebrates. This is in agreement with the result arrived at by van Wijhe in a totally different way, which states that the abdominal pores were excretory ducts and the body-cavity the most primitive excretory organ, functioning as such before the pronephros arose.* It would follow that the abdominal pore is phylogenetically older than pronephric and mesonephric segmental tubes and ducts, and Balfour's homology of the abdominal pores with a posterior pair of segmental tubes would fall to the ground. Without committing myself to van Wijhe's way of regarding the abdominal pores and pronephros, I must say that there is very little to be alleged in favour of Balfour's homology. The arguments with which Balfour† supports his view are rather scanty, amounting to the statement that the pores, for reasons given, are not Müllerian ducts, and that the blind pockets (cloacal pouches) of Selachians are very like primitive involutions from the exterior to form the external openings of a pair of segmental organs. It is now known that the peritoneal end of segmental tubules is formed, especially in Selachians, in a perfectly definite manner from a definite part of the myotome, the dorsal portion of ·that part which does not form the myomere or muscle segment. Until it is shown that the abdominal pores arise from a corresponding portion of a myotome they cannot be homologised with segmental organs. Their late appearance in many cases, their ventral position, the fact that they are formed (in Scyllium for instance) at the extreme tip of the peritoneal cavity as a prolongation of that cavity ventrally into the cloacal papilla, that they open at or near the tip of the papilla and not into the bottom of the cloacal pouch, all these facts make it seem unlikely that Balfour's suggested homology will eventually be proved.

The problem naturally arises, which is the primitive condition in Elasmobranchs, that where nephrostomes alone are present, or that where abdominal pores alone are present in the adult? This point cannot, I think, be decided without more evidence.

At first sight it might seem that the persistence of nephrostomial tubes in the adult Selachian was primitive, as these organs are formed at an early stage, in a primitive manner, and are hence phylogenetically ancient structures. Cestracion, moreover, which has this feature, is one of the most primitive of living Selachians. But against all this it may be urged that just as the nephrostomes in adult Amphibia are in all probability a neotenic character, in a group which shows so strong a proclivity to neotenia, so may the retention of open nephrostomes in adult Selachians of the present day be a neotenic phenomenon. This is the more probable, since the nephrostomes in animals which lose them generally disappear at a

* 'Arch. f. Mikr. Anat.,' vol. 33, p. 507.
† Memorial Edition, vol. 1, p. 153.

very early age and they are so lost in the Cyclostomata. It is, therefore, not clear whether that intermediate group of Selachians which has both nephrostomes and abdominal pores is on the way to lose the former and depend on the latter or *vice versâ*.

With regard to the homology of the pores in the different groups of fishes and in reptiles, the answer will depend on the proof or disproof of Balfour's homology. If the pores are not segmental tubes, they are simply perforations of the abdominal wall in consequence of gradual thinning down in the cloacal region. Should this be so, it is evident that this process may have taken place independently many times over in the phylogeny of the different groups, and there would be great difficulty in establishing the homology between any two groups of Vertebrates in respect to the pores.

Let us glance for a moment at the *rôle* played by the body-cavity in the series of changes from its original condition (i) as a part of the excretory system, (ii) as a part of the reproductive system receiving the genital products as they are set free, and (iii) as a part of the lymphatic system, receiving the transudations of the visceral and abdominal walls. From this condition it has been specialised, losing first the sexual part of its duties, when the ovaries and testes became more or less continuous with their ducts. It would afterwards, as in the Anura, become less an excretory organ and more a lymph reservoir, and similarly in the higher reptiles and mammals it becomes more and more specialised as a part of the system of lymphatics.

I must record my gratitude to Mr. Boulenger for facilities granted in examining specimens under his charge at the British Museum, to Mr. S. F. Harmer, Superintendent of the Museum of Comparative Anatomy, Cambridge, for the use of specimens in the collection, and lastly, I owe much gratitude to Dr. Hans Gadow, for the encouragement and suggestive assistance he has always been ready to give me.

Summary.

1. There is a reciprocal and compensating correlation in the adult Elasmobranchii, Ganoidei, Dipnoi, some Teleostei, Amphibia, certain Chelonia and Crocodilia in the distribution of nephrostomes and of abdominal pores. In some Selachians only are both present. In the majority of the Elasmobranchii, and in all the other groups, the presence of one set of organs excludes the presence of the other. In the higher Teleostei, in Hatteria, some Crocodiles and Chelonians, both have been lost. Anura hold an intermediate position in so far as the nephrostomes are present, but are no longer connected with the renal system, and the body-cavity communicates with the circulatory system through two channels, (1) through the nephrostomes,

and (2) through stomata. The latter alone form a communication between the body-cavity and the lymphatic system in the Saurii, and Mammalia which have neither abdominal pores nor nephrostomes.

2. If stomata should not be present in the Teleostei and certain Reptilia mentioned above, they would form the only exception to the generalisation that the body-cavity of Vertebrates is never completely closed.

3. The function of the abdominal pores is the same as that of the nephrostomes, viz., the voiding of waste products from the body-cavity.

4. The body-cavity in Pisces and the lower Amphibia is to a great extent an excretory organ.

5. The bulk of the available evidence does not favour the view that the abdominal pores represent a pair of segmental tubules.

6. They seem to be simple perforations of the abdominal wall, and in this case would not necessarily be homologous in the various groups of Vertebrates.

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[*] References already given in the text are not repeated here.

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"On the Calculation of the Coefficient of Mutual Induction of a Circle and a Coaxial Helix, and of the Electromagnetic Force between a Helical Current and a Uniform Coaxial Circular Cylindrical Current Sheet." By J. VIRIAMU JONES, F.R.S. Received November 12,—Read December 9, 1897.

(Abstract.)

§ 1. Let M_Θ be the coefficient of mutual induction of a circle and a portion of a coaxial helix, beginning in the plane of the circle and of helical angle Θ. Then if M is the coefficient of mutual induction of the circle, and any portion of the helix for which the extreme points are determined by helical angles Θ_1 and Θ_2, we have

$$M = M_{\Theta_2} - M_{\Theta_1}.$$

It will therefore be sufficient to show how to calculate M_Θ for all values of Θ.

Let the equations to the circle and coaxial helix be

$$y = a \cos \theta \left.\right\} \quad y' = A \cos \theta' \left.\right\}$$
$$z = a \sin \theta \left.\right\} \quad z' = A \sin \theta' \left.\right\}.$$
$$x = 0 \left.\right\} \quad x' = p\theta' \left.\right\}$$

Then it has been shown by the author[*] that M_Θ may be expressed by the following series which is convergent if $x < A - a$

$$M_\Theta = \Theta(A+a)c^2 \Sigma(-1)^{m+1} \frac{1.3.5.\ldots(2m-1)}{2.4.6.\ldots2m} \frac{1}{2m+1} \left(\frac{x}{A+a}\right)^{2m} P_m,$$

where

$$c = \frac{2\sqrt{Aa}}{A+a}, \quad x = p\Theta,$$

$$P_m = \int_0^{\frac{\pi}{2}} \frac{\cos 2\theta \, d\theta}{(1 - c^2 \sin^2\theta)^{\frac{2m+1}{2}}}.$$

[*] 'Phil. Mag.,' January, 1889.

§ 2. Putting $K_m = -\dfrac{1.3.5. \dots (2m-1)}{2.4.6. \dots 2m} \dfrac{1}{2m+1} \left(\dfrac{x}{A+a}\right)^{2m} P_m,$

so that $M_\Theta = \Theta(A+a)c^2 \Sigma(-1)^m K_m$

it is now further proved that

$$K_{m+1} = \frac{2m(2m+1)}{(2m+2)(2m+3)} d^2 \left\{ K_m - \frac{(2m-1)(2m-3)}{2m \cdot 2m} e^2 K_{m-1} \right\}$$

where $d^2 = \dfrac{1+c'^2}{c'^2} \left(\dfrac{x}{A+a}\right)^2$

$$e^2 = \frac{1}{1+c'^2} \left(\frac{x}{A+a}\right)^2$$

$$c'^2 = 1-c^2.$$

Hence we see that the series is a recurring series. The relation above given between K_{m+1}, K_m, and K_{m-1} materially facilitates the calculation of successive terms of the series.

§ 3. If we put

$$\Sigma(-1)^m K_m = W$$

$$\Sigma(-1)^m m K_m = T$$

$$\Sigma(-1)^m \frac{m}{2m-1} K_m = V$$

and $\dfrac{dM}{M} = q\dfrac{dA}{A} + r\dfrac{da}{a} + s\dfrac{dx}{x},$

then $q = \dfrac{1-s}{2} + \dfrac{T+2V}{deW}$

$$r = \frac{1-s}{2} - \frac{T+2V}{deW}.$$

$$s = 2T/W.$$

§ 4. The following is a more general expression of M_Θ in terms of elliptic integrals applicable for all values of x :—

$$M_\Theta = \Theta(A+a)ck \left[\frac{F-E}{k^2} + \frac{c'^2}{c^2}(F-\Pi) \right]$$

where $k = \dfrac{2\sqrt{Aa}}{\sqrt{(A+a)^2 + x^2}}$

$$F = \int_0^{\frac{\pi}{2}} \frac{d\theta}{\sqrt{1-k^2 \sin^2 \theta}}$$

$$E = \int_0^{\frac{\pi}{2}} \sqrt{1-k^2 \sin^2 \theta} \cdot d\theta$$

$$\Pi = \int_0^{\frac{\pi}{2}} \frac{d\theta}{(1 - c^2 \sin^2\theta)\sqrt{1 - k^2 \sin^2\theta}}.$$

The elliptic integral of the third kind Π is expressible by means of Legendre's formula* in terms of elliptic integrals of the first and second kind, complete and incomplete, and may so be calculated without difficulty.†

§ 5. Putting as before

$$\frac{d\mathrm{M}}{\mathrm{M}} = q\frac{d\mathrm{A}}{\mathrm{A}} + r\frac{d\iota}{a} + s\frac{dx}{x},$$

it is shown that

$$q = \frac{\mathrm{A}}{2a\mathrm{U}}(\mathrm{F} - c'\Pi)$$

$$r = \frac{a}{2\mathrm{A}\mathrm{U}}(\mathrm{F} + c'\Pi)$$

$$s = -1 - \frac{1}{\mathrm{U}}\left\{\left(1 - \frac{2}{k^2}\right)\mathrm{F} + \frac{2}{k^2}\mathrm{E}\right\}$$

where

$$\mathrm{U} = \frac{\mathrm{F} - \mathrm{E}}{k^2} + \frac{c'^2}{c^2}(\mathrm{F} - \Pi) = \frac{\Theta(\mathrm{A} + a)ck}{\mathrm{M}}.$$

§ 6. The mutual potential energy of a circular current and a uniform coaxial circular cylindrical current sheet (the current lines being in planes at right angles to the axis) is identically the same as the mutual potential energy of the circular current and a coaxial helical current of the same radial and axial dimensions, beginning and ending in the ends of the sheet, if the current across a length of a generating line of the sheet equal to the pitch of the helix is equal to the helical current.

§ 7. The mutual potential energy of a helical current and a uniform coaxial circular cylindrical current sheet, or of two uniform coaxial circular cylindrical current sheets is expressible in terms of elliptic integrals.

§ 8. The electromagnetic force between a helical current and a uniform coaxial circular cylindrical current sheet is given by the formula

$$\mathrm{F} = \gamma_h\gamma(\mathrm{M}_2 - \mathrm{M}_1)$$

where γ_h = the current in the helix,

γ = the current across unit length of a generating line of the sheet,

* 'Cayley,' "Elliptic Integrals," § 158.
† 'Cayley,' chap. 13.

and M_1, M_2 are the coefficients of mutual induction of the circular ends of the sheet and the helix.

Hence the calculation of this force reduces itself to a double application of the formulæ for the coefficient of mutual induction of a circle and coaxial helix.

It is hoped that this may form a useful method of calculating the constant of current weighers designed to measure current in absolute units.

" A Note on some further Determinations of the Dielectric Constants of Organic Bodies and Electrolytes at very Low Temperatures." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution. and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London.   Received October 28,—Read December 9, 1897.

In several previous communications[*] we have described the investigations made by us on the dielectric constants of various frozen organic bodies and electrolytes at very low temperatures. In these researches we employed a method for the measurement of the dielectric constant which consisted in charging and discharging a condenser, having the given body as dielectric, through a galvanometer 120 times in a second by means of a tuning-fork interrupter. During the past summer we have repeated some of these determinations and used a different method of measurement and a rather higher frequency. In the experiments here described we have adopted Nernst's method for the measurement of dielectric constants, using for this purpose the apparatus as arranged by Dr. Nernst which belongs to the Davy-Faraday Laboratory. The frequency of alternation employed was 350 or thereabouts, whereas in all our formerly described experiments with the galvanometer method it was 120.

The electrical details of the arrangement employed in Nernst's method are as follows:—A Wheatstone's bridge is formed (see diagram), two sides of which consist of variable resistances, R_1, R_2, which are usually liquid resistances contained in $\cup$-tubes. The other two sides of the bridge consist of two sliding condensers of variable capacity, C_1, C_2, which are shunted by adjustable liquid resistances, R_3, R_4. The bridge circuit contains a telephone, T, as detector. The alternating currents are furnished by an induction coil, I. An experimental condenser, X, the dielectric of which can be made

* See Fleming and Dewar, ' Roy. Soc. Proc.' (1897), vol. 61, pp. 2, 299, 316, 358, 368, and 381.
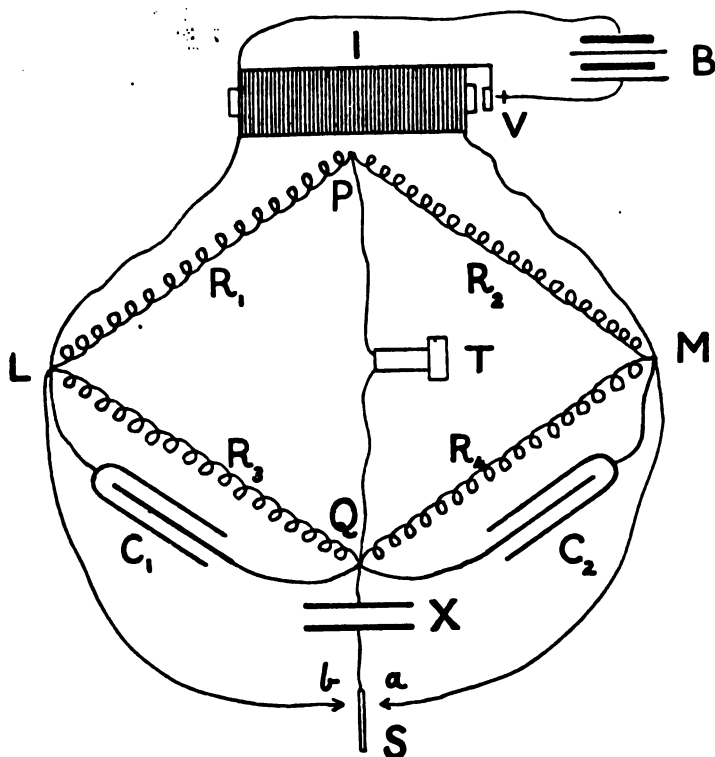
to be the substance under examination, is connected, as shown in the diagram, so that it can be placed in parallel with either of the shunted condensers forming the third and fourth arms of the bridge. The process of measurement is then as follows:—The experimental condenser is placed in parallel, by the switch S, say, with the third arm of the bridge, and the shunt resistances are first adjusted, so that the telephone gives a minimum of sound. The capacity of one sliding condenser, C_1, is then varied until complete silence in the telephone in the bridge is obtained. The experimental condenser is next shifted over into parallel with the fourth arm, and the capacity of the same sliding condenser is again adjusted to produce silence in the telephone. The change in capacity thus made in the sliding condenser corresponding to the change in position of the experimental condenser is denoted by s. The experimental condenser then has its air dielectric replaced by a liquid of known dielectric constant D_0, and the same process of change of its position effected. Let the variation of the adjustable sliding condenser then be denoted by S_0. Finally the experimental condenser has its air dielectric replaced by a liquid or frozen liquid of unknown dielectric

constant D, and the process of change and adjustment a third time repeated. Let the variation of the sliding condenser in this third case be S. It is then very easy to show that the following relation holds good between D_0, D, S_0, S, and s, viz. :—

$$\frac{D_0-1}{S_0-s} = \frac{D-1}{S-s}$$

or
$$D = \frac{D_0-1}{S_0-s}(S-s)+1.$$

In order to apply this method we employed absolute ethylic alcohol as the standard dielectric substance of known dielectric constant, and took as its dielectric constant at 15° C. the value 25·8 (according to Nernst), a number closely in agreement with all the best results by other observers.

The actual capacity of the experimental condenser with air as dielectric was very small, not being more than about 0·00001 of a microfarad. Hence in the above formula $D_0 = 25·8$.

The value of s was determined to be 1·33 and 1·38 in two experiments, and the mean 1·36 was taken as the value of s.

The following liquids were then examined by placing them in the experimental condenser and keeping them at ordinary temperatures, viz., 16° C. to 20° C.

1. Solution of Potassic Hydrate in water, 5 per cent. solution.
2. Solution of Rubidic Hydrate in water, 5 per cent. solution.
3. Amyl alcohol.
4. Ethylic ether.
5. Ethylic ether, pure and dry.
6. Ethylic alcohol.

The change in capacity of the sliding condenser when ethylic alcohol replaced the air in the experimental condenser was 16·7. Hence $S_0 = 16·7$ and $D_0 = 25·8$, also $s = 1·36$.

Therefore
$$\frac{D_0-1}{S_0-s} = \frac{24·8}{15·34} = 1·613.$$

The following table shows the observed values of s in the several cases when the above liquids were placed in the experimental condenser, and the corresponding calculated value of the dielectric constant D, where $D = 1·613 \times (S-s)+1$.

Table I.—Determinations of the Dielectric Constants of certain Liquids at Ordinary Temperatures (15° C.) by Nernst's Method. Frequency = 320.

Substance.	S.	S−s.	1·613 (S−s).	Dielectric constant = D.
Ethylic alcohol (taken as the standard of comparison)	16·7	15·34	24·8	25·8 (assumed value)
Amyl alcohol	10·49	9·13	14·7	15·7 (calculated)
Ethylic ether........	3·98	2·62	4·23	5·23 (calculated)
Pure dry ethylic ether	3·70	2·34	3·78	4·78 (calculated)

Hence by Nernst's method, assuming the dielectric constant of ethylic alcohol to be 25·8, we find that of amyl alcohol to be 15·7 and pure ethylic ether to be 4·78.

Nernst himself found amyl alcohol to be 16·0 and ethylic ether to be 4·25 at about this temperature. Hence our values are in fair agreement with his.

In the next place we cooled the experimental condenser down to the temperature −185° C. in liquid air, after filling it with one of the above six dielectric liquids, and we repeated all the above-described operations again. The results are collected in Table II.

Table II.—Determinations of the Dielectric Constants of certain Frozen Liquids at the Temperature of Liquid Air by Nernst's Method. Frequency = 320.

Substance.	S.	S−s.	1·613 × (S−s).	Calculated dielectric constant = D.
Ethylic alcohol	2·68	1·32	2·13	3·13
Amyl alcohol	2·34	0·98	1·58	2·58
Ethylic ether	2·16	0·80	1·29	2·29
5 per cent. solution of Rubidic Hydrate	2·94	1·58	2·55	3·55
5 per cent. solution of Potassic Hydrate	5·15	3·79	6·12	7·12

The above values for the organic bodies are in close agreement with the results we obtained for the same substances by the galvano-meter and switch method formerly used by us, as may be seen by a reference to Table III.

Table III.—Comparison of the Determinations of certain Dielectric Constants made by different Methods at the Temperature of Liquid Air.

Substance.	By galvanometer and switch method. Frequency = 120. Dielectric constant.	By Nernst's bridge method with telephone. Frequency = 320. Dielectric constant.
Ethylic alcohol..............	3·11	3·13
Amyl alcohol..............	2·14	2·58
Ethylic ether................	2·31	2·29
5 per cent. solution (aqueous) of Potassic Hydrate......	123·0	7·12
5 per cent. solution (aqueous) of Rubidic Hydrate	81·6	3·55

The results collected in the above Table III, show that the two methods give practically identical values for the two alcohols and the ether, but very different value for the two frozen dilute hydrates

An examination was then made of several other substances, and for this purpose another condenser was constructed, which consisted of a platinum crucible about 4 cm. in diameter and 5 cm. high. This crucible was fitted with an ebonite lid, through which passed a glass test-tube, in the interior of which was placed our platinum thermometer. Round the outside of the test-tube, platinum wire was closely wound, so as to form the opposed surface of a condenser in relation to the platinum crucible as the other surface. This platinum condenser could then be filled with any electrolyte or organic liquid and frozen in liquid air.

Owing to the very small actual capacity of this last experimental condenser, and especially that of the variable part of it in comparison with the capacity of the leads and connections, no very great accuracy of measurement was looked for or attained. The results, however, were sufficient to check the general [accuracy of the experiment with similar substances by the galvanometer method. This platinum condenser was calibrated and used with the Nernst bridge, exactly as in the previous experiment.

With the experimental condenser empty the change in capacity of the variable sliding condenser in the bridge arm was 1·50 on changing over the position of the experimental condenser. Hence $s = 1·50$.

When filled with ethylic alcohol ($D_0 = 25·8$) the change of capacity of the sliding condenser was 6·20. We have, therefore, $S_0 = 6·20$, $D_0 = 25·8$, $s = 1·50$.

Therefore

$$\frac{D_0 - 1}{S_0 - s} = \frac{24·8}{4·70} = 5·27.$$

The experimental condenser was then filled with some liquid, either at ordinary temperature or frozen at a low temperature, and

the bridge measurement made, and the reading S or the change of capacity of the sliding condenser observed, as before, when the experimental condenser had its position changed.

The following Table IV gives the summary of the results obtained with several substances at various temperatures.

Table IV.—Measurement of the Dielectric Constants of various Substances at different Temperatures by Nernst's Method. Frequency = 320. $S = 1.50$, $\dfrac{D_0 - 1}{S_0 - s} = 5.27$.

Substance.	S.	S−s.	5·27(S−s).	Calculated dielectric constant = D.	Temperature in platinum degrees.
Ethylic ether..	2·1	0·6	3·16	4·16	+15°
Glycerine	12·15	10·65	56·2	57·2	+30
Solution of am-	4·5	3·0	15·8	16·8	−123 (?)
monia (sp. gr.	5·6	4·1	21·6	22·6	−137
= 0·880)....	13·5	12·0	63·0	64·0	−119
Distilled water	0·2	0·5	2·6	3·6	−49
or pure ice ..	11·0	9·5	50·0	51·0	−7
	14·3	12·8	67·0	68·0	+1
Oxide of copper	3·95	2·45	12·9	13·9	−61
in suspension in water....	12·8	11·3	59·7	60·7	−41

The value found for the dielectric constant of water at +1° is rather low, but, as above mentioned, the smallness of the capacity of the experimental condenser prevented the results from being more than good indications of the *order* of the dielectric constant.

We have, in addition, repeated and extended experiments made with the cone condenser on various electrolytes and dielectrics.

In the first place, we have carefully examined the effect of change of temperature on the dimensions of the cone condenser *per se* to ascertain if the dimensional change produced by cooling it in liquid air could sensibly affect the value of the dielectric constant of an electrolyte forming the dielectric between the cones, apart from the change which temperature produces in the dielectric quality of the dielectric itself.

The gilt cone condenser was accordingly connected with the tuning-fork interrupter as formerly described,* and the galvanometer scale deflection when the condenser was charged with 97·2 volts was found to be 3·85 cm. to the left and 3·88 cm. to the right, hence the mean galvanometer deflection was 3·87 cm. of the scale. This corrected to 100 volts becomes 3·98, and deducting 0·4 cm. for the capacity of the leads, gives 3·58 as the number representing the

* See Fleming and Dewar, 'Roy. Soc. Proc.,' vol. 61, 1897, p. 299.

electrical capacity of the cone condenser as then arranged. A standard condenser belonging to the Davy-Faraday laboratory, and having a capacity of one-thousandth of a microfarad, was then substituted for the cone condenser, and gave right and left deflections of 19·1 and 19·6 cm. respectively when charged with 97 volts. This corrected for capacity of leads (= 0·4 cm.) and reduced to 100 volts becomes 19·53. Hence, since the electrical capacities are proportional to the galvanometer deflection, the electrical capacity of our cone condenser is $\frac{3\cdot58}{19\cdot53}$ × 0 001 of a microfarad, or 0·000183 of a microfarad.

The capacity of the gilt cone condenser was then again measured with air at 20° C. as dielectric, and the galvanometer deflection observed. The outer cone was then cooled to −185° C. by quickly applying to it a large quantity of liquid air whilst the inner cone remained at about 20° C., and the galvanometer deflection again observed. This deflection was taken again when the inner cone had fallen in temperature to −75°, and finally when both inner and outer cones were at −185° C. The following numbers giving the galvanometer deflections are then proportional to the electrical capacity of the condenser between the cones.

Table V.—Examination of the Effect of Cooling on the Electrical Capacity of the Cone Condenser.

Galvanometer scale deflection in cm. or electrical capacity of the cone condenser in arbitrary units, the dielectric being gaseous air.	Remarks.
4·22 cm.	Both inner and outer cones both at 20° C.
4·82 cm.	Outer cone at −185° C. Inner cone at 20° C., about.
4·58 cm.	Outer cone at −185° C. Inner cone at −75° C.
4·19 cm.	Outer and inner cones both at −185° C.

Hence it is clear that mere change of temperature of the metal work of the cone condenser does not affect its electrical capacity by more than 1, or perhaps 2, per cent., and any larger changes in capacity found on cooling must be due to a real change in the dielectric constant of any dielectric substituted for the air between the cones, and not to mere dimensional changes of the condenser itself produced by the cooling.

Another matter to which our attention was directed was the

question whether there was any sensible or serious lag in the temperature of the resistance thermometer behind the temperature of the dielectric. Our usual custom had been to immerse the condenser when prepared for use in liquid air, and cool down the whole mass to $-185°$ C., and then raising it out of the liquid air to take temperature and capacity readings as it warmed up. The resistance thermometer was placed in the inner cone in a thin test-tube, and fixed in with fusible metal in the inner cone. We therefore tried one experiment with pure glycerine as dielectric, in which the electrical measurements were made as the condenser was slowly cooled, instead of being made as it slowly heated up. The process of cooling from $-38°$ *pt.* to $-201°$ *pt.* was allowed to occupy one hour and forty minutes. The values thus found for the dielectric constant for glycerine for this range of temperature were practically in agreement with those found when the condenser capacity was measured as it warmed up instead of cooled down.

Table VI.

I. *Dielectric Constant of Pure Glycerine.*

Corrected galvanometer deflection when the condenser had air as dielectric = $3·92$ cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
$-38·0$	$2·85$	$50·5$	Condenser charged with $1·434$
$-42·6$	$2·97$	$52·8$	volts. Time = 3.5 P.M.
$-46·0$	$3·00$	$53·3$	
$-55·8$	$3·17$	$56·5$	
$-64·8$	$3·03$	$53·8$	Time = 4.15 P.M.
$-98·8$	$2·70$	$3·95$	Condenser charged with $17·0$ volts.
$-119·5$	$2·20$	$3·19$	Time = 4.30 P.M.
$-201·0$	$11·05$	$2·82$	Condenser charged with $92·4$ volts. Time = 4.45 P.M.

The above values of the dielectric constant of glycerine are in very fair agreement with the values obtained by us during rising temperature.[*]

We have also extended our former observations made with this cone condenser and the galvanometric method to certain other frozen electrolytes, and measured their dielectric constants at low temperatures.

The results and substances are as follows :—

[*] ' Roy. Soc. Proc.,' vol. 61, p. 324.

II. *Dielectric Constant of Dry Concentrated Sulphuric Acid* (H_2SO_4).

The corrected galvanometer deflection with air as dielectric was 3·92 cm. for 100 volts.   The switch frequency was 120.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·9	14·6	3·86	Condenser charged with 94·2 volts.
−186·0	14·8	3·90	,,　　,,　　,,　　,,
−181·8	2·5	3·82	Condenser charged with 16·2 volts.
−150·7	2·7	4·13	,,　　,,　　,,　　,,
−129·8	2·95	4·33	Condenser charged with 16·9 volts.
−110·0	6·0	7·25	Condenser charged with 17·0 volts.

III. *Dielectric Constant of Dry Concentrated Nitric Acid* (NO_3H).

Corrected galvanometer deflection with air as dielectric = 3·90 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·7	9·05	2·36	Condenser charged with 94·2 volts.
−182·7	9·27	2·46	
−165·8	9·25	2·42	
−118·2	9·40	2·45	
−129·7	10·00	2·62	

Before carrying out these experiments with the concentrated nitric and sulphuric acids the condenser had been carefully re-gilt and a glass steady-pin substituted for the ebonite one.

IV. *Dielectric Constant of Sodium Fluoride* (NaF).

(10 per cent. solution.)

Corrected galvanometer deflection with air as dielectric = 4·04 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·3	9·3	2·33	Condenser charged with 94·2 volts.
−174·2	9·3	2·35	
−149·2	10·35	2·55	
−135·0	14·35	3·67	
−125·0	19·6	5·05	
−115·7	4·4	7·07	Condenser charged with 15·2 volts.

The electrical resistance of the condenser at − 200 *pt.* with frozen sodic fluoride as dielectric was 2000 megohms.

V. *Dielectric Constant of Hydrosodic Fluoride* (NaFHF).

(10 per cent. solution.)

Corrected galvanometer deflection with air as dielectric = 3·42 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·8	8·87	2·28	
−186·0	8·87	2·25	
−169·0	8·95	2·30	
−178·8	9·0	2·31	
−148·8	9·7	2·49	
−142·2	10·9	2·79	
−131·5	15·4	4·02	

VI. *Dielectric Constant of Sodium Peroxide* (Na$_2$O$_2$).

(5 per cent. solution.)

Corrected galvanometer deflection with air as dielectric = 4·41 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−198·0	4·5	71	Condenser charged with 1·434 volts.
−184·5	5·5	87	

VII. *Dielectric Constant of Solution of Hydroxyl* (H$_2$O$_2$).

(20 per cent. solution by volume.)

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−203·5	10·8	2·38	Condenser charged with 99·2 volts.

Some experiments were then made by electrolysing freely certain electrolytes and freezing them with liquid air in the act of electrolysis. The dielectric constants were then subsequently determined in the frozen state.

VIII. *Dielectric Constants of Frozen Electrolytes, Electrolysed freely in the act of Freezing.*

Corrected galvanometer deflection with air as dielectric = 3·42 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
			(*a*) 5 per cent. aqueous solution of potassic hydrate electrolysed with 0·2 ampere and 8 volts. Evolved gas = 5·1 c.c.
−200·0	3·85	71·4	Condenser charged with 1·434 volts. Electrical resistance of condenser 5000 megohms when frozen.
			(*b*) Water electrolysed with 1·0 ampere.
−198·2	8·70	2·47	Condenser charged with 98 volts. Electrical resistance of condenser 5000 megohms when frozen.
			(*c*) Water electrolysed with 2·1 amperes.
−198·0	8·65	2·42	Condenser charged with 98·9 volts.

It is evident that the action of electrolysis prior to, and during freezing has no sensible effect on the subsequently measured dielectric constant even though the surfaces of the cone condenser are strongly polarised in the act of freezing the liquid dielectric.

We have then paid some attention to the possible cause of the high dielectric values of some substances at very low temperatures. It is clear from the above described experiments with the Nernst bridge that for organic bodies such as ethylic alcohol, amyl alcohol, ethylic ether, and glycerine we obtain practically the same dielectric values at the low temperature, both when they are measured by the galvanometric method with a switch frequency of 120, and when measured by Nernst's method with a frequency of 350, or about three times as great.

On the other hand for certain other bodies such as the frozen dilute hydrates of potassium and rubidium, and the oxide of copper suspended in ice, the dielectric value at the low temperatures is much diminished by increasing the frequency.

Subsequently to the completion of the experiments described in this paper the suggestion has been made by R. Abegg* that the high dielectric values at low temperatures are due to polarization of the

* 'Wied. Ann.,' vol. 62, p. 249.

electrodes of the condenser, and that the capacity measured is a polarisation capacity and not a true dielectric capacity, and he supports this contention by pointing out that whenever we have obtained a large dielectric value at the low temperature it has always been measured with an electromotive force of 1·434 volts, which is less than the ordinary full reverse electromotive force of polarization.

There are, however, reasons for considering that this contention is not a valid one. In the first place we have always in all the measurements begun operations by testing the dielectric capacity of our condenser with an electromotive force of one Clark cell (= 1·434 volts) in order to see roughly whether the dielectric value was large or small. If it was a small value we then gradually increased the electromotive force until a good readable galvanometer deflection was obtained. We never found that with an electromotive force of 1·434 volts, the dielectric constant of any substance was greater than with a much higher voltage.

In the next place, in many cases we changed from a working electromotive force of about 20 volts to one of 1·434 volts, and we never found any marked discontinuity in the calculated value of the dielectric constant at that point. If our previous papers on this subject are examined the following instances may be found.

Table VII.—Measurement of various Dielectric Constants with different Electromotive Forces.

Substance.	Tempera-ture.	Dielectric. constant.	Voltage with which con-stant was measured.	Reference.
Ice .... $\left\{\begin{array}{l}\\ \\\end{array}\right.$	− 89·4 − 87·2	27·6 29·0	19·8 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ 'Roy. Soc. Proc.,' vol. 61, p. 318.
„ .... $\left\{\begin{array}{l}\\ \\\end{array}\right.$	−124·2 −119·2	18·1 21·8	20·3 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ *Ibid.*, p. 321.
Sodic chloride, 10 p. c. solution.. $\left\{\begin{array}{l}\\ \\\end{array}\right.$	−118·5 −114·0	21·2 22·3	20·2 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ *Ibid.*, p. 307.
Potassic chromate, 30 p. c. solution.. $\left\{\begin{array}{l}\\ \\\end{array}\right.$	−112·4 −100·0	16·9 22·3	20·0 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ *Ibid.*, p. 387.
Cupric carbonate, 10 p. c. suspension $\left\{\begin{array}{l}\\ \\\end{array}\right.$	−124·7 −120·8	16·5 21·8	18·2 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ *Ibid.*, p. 390.
Baric hydrate, 5 p. c. suspension $\left\{\begin{array}{l}\\ \\\end{array}\right.$	−178·0 −174·2	23·9 25·5	19·5 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ *Ibid.*, p. 372.
Bismuth oxide, 10 p. c. suspension $\left\{\begin{array}{l}\\ \\\end{array}\right.$	−129·2 −127·3	19·9 24·5	17·8 1·4	$\left.\begin{array}{l}\\ \\\end{array}\right\}$ *Ibid.*, p. 376.

An examination of the above instances will show that if the electromotive force is changed from about 20 volts to 1·4 volts, it does

not make a greater difference in dielectric constant than can be properly ascribed to the accompanying change in temperature.

[Again the following measurements were made at about the same voltage :—

Substance.	Temperature.	Dielectric constant.	Charging voltage.
Potassic Bicarbonate ...	−166·5	2·80	19·8 ⎱
Sodic Bicarbonate	−166·7	48·70	19·8 ⎰
Ferric Chloride........	−133·8	4·23	19·8 ⎱
Sodic Chloride.........	−129·2	14·55	20·2 ⎰
Cupric Carbonate	−132·7	3·42	18·2 ⎱
Cupric Sulphate	−133·2	16·40	19·7 ⎰
Lithic Hydrate	−198·0	3·23	19·8 ⎱
Baric Hydrate	−196·8	20·10	19·5 ⎰

It is unlikely that polarisation accounts for the differences between the dielectric constants of the above substances, taken pair and pair, when measured at nearly identical temperatures, and very nearly the same voltages.—*November* 8, 1897.]

In order to settle the matter finally we propose, however, to re-measure a number of those substances which have shown high dielectric values at the low temperature when measured by the galvanometric method at a frequency of 120 but using in all cases an electromotive force of 100 volts.

If under the larger electromotive force the dielectric values of some electrolytes still remain large, it will be difficult to ascribe this large value to polarization.

The facts, however, admit of another interpretation. It is clear that the dielectric constants of some substances at low temperatures are vastly more susceptible to change of electromotive force fre-quency than is the case with others, and that the electric strain pro-duced by a given electric stress varies in some cases enormously with the time of imposition of the stress but very little in others.

Another argument against the view that these high dielectric values are due to polarisation, as ordinarily understood, is as follows : The results of most numerous experiments on water show that the dielectric constant at or near 0° C., is a number not far from 80. This value is obtained whether the electromotive force reversals are infinitely slow or whether they are very large. The results of the measurement of the electrical refractive index of water even with ether waves only 4 mm. in length, and, therefore, having a fre-quency of about $7·5 \times 10^{10}$, as given recently by Lampa,[*] indicate a number not far from 9·5 as the refractive index and hence give a dielectric value of 90. There can be no question of polarisa-tion of electrodes in this last case. On the other hand an increase

[*] 'Wien. Ber.,' Part 2*a*, p. 587, 1896 ; also p. 1049, 1897.

in frequency which hardly affects the value of the dielectric constant of water is sufficient to greatly decrease that of ethylic alcohol, and at the same frequency the dielectric constant of ethylic alcohol was found by Lampa to have fallen to a value of 5. Hence ethylic alcohol is more sensitive to change of frequency than water. There is, therefore, no *a priori* reason why we might not expect to find the same thing even in a much greater degree in certain other bodies at lower temperatures, viz., a true high dielectric value for a certain frequency, but great sensitiveness to increase of frequency in such fashion that increase of frequency greatly reduces the dielectric value. At the present stage of the enquiry it seems undesirable to endeavour to regard the facts wholly from the point of view of one hypothesis as to the nature of electrolysis.

We have again to mention with pleasure the assistance we continue to receive from Mr. J. E. Petavel in the observational part of these investigations.

Note added December 8, 1897. Received December 9, 1897.

Since the above paper was printed we have repeated some of our former experiments with the 5 per cent. solution of rubidic hydrate and the 5 per cent. solution of potassic hydrate, using the original method in which a condenser having the frozen electrolyte as dielectric is charged and discharged through a galvanometer, but employing much higher charging voltages. The object of these experiments was to apply a further test as to the validity of the contention put forward by R. Abegg, that we have obtained high dielectric values for certain of these frozen electrolytes in consequence of having invariably used an electromotive force of 1·434 volts in the experiments with these particular substances. In order to be able to work with larger electromotive forces we arranged three galvanometers of the Holden d'Arsonval type otherwise exactly similar, except in having different sensibilities and resistances. One was the 500-ohm galvanometer used in all our previous condenser experiments, another was a 100-ohm coil galvanometer, and a third was a 4-ohm coil galvanometer. These were used at the same distance (125 cm.) from the scale as formerly. An approximate test for the relative sensibility of these galvanometers was made by placing a Clark standard cell in series with each galvanometer through 100,000 ohms and noting the scale deflection produced. As the internal resistance of the galvanometers was at most only $\frac{1}{2}$ per cent. of the total resistance these scale deflections may be considered to be approximately produced by the same current. The scale deflections in centimetres were—

For the 500-ohm galvanometer.... 37·3 cm.

  „ 100-ohm „ .... 6·7 „

  „ 4-ohm   „ .... 0·55 „

Hence, the sensibilities are in the ratios of these deflections, and the deflections of the 100-ohm galvanometer must be multiplied by 5 5, and those of the 4-ohm galvanometer by 67·8, to reduce their scale readings to equivalent deflections in terms of the 500-ohm galvanometer.

The 500-ohm galvanometer was then used with the condenser and vibrator, as described in one of our previous papers.* The condenser having gaseous air as dielectric, the scale deflection for a frequency of 120 and an electromotive force of 97 volts was 3·2 cm. when corrected for capacity of leads.

The condenser then had its dielectric space filled with the 5 per cent. solution of rubidic hydrate, and was frozen in liquid air. The dielectric constant was next measured, using an electromotive force of 17·8 volts and the 500-ohm galvanometer. The value of the dielectric constant found, when corrected for the capacity of the leads, was 65·6. The mean corrected scale deflection was 38·5 cm. for 17·8 volts. This, reduced to its equivalent for 97 volts, is 210 cm. and 210/3·2 = 65·6.

In the next place the same experiment was repeated employing the 100-ohm galvanometer and an electromotive force of 79 volts. Applying the necessary corrections to the observed scale deflection of 23·5 cm., and reducing to the equivalent deflection on the 500-ohm galvanometer, gave 160 cm. as the value of the reduced deflection. Hence 160/3·2 = 50 is the dielectric constant. The rather considerable difference between these values (65·6 and 50) is not a matter for surprise, having regard to the extreme steepness of the dielectric curve of the rubidic hydrate solution at about the temperature of liquid air. As we were merely desirous of determining whether a considerable increase of electromotive force would greatly diminish the large dielectric value, we did not trouble to put in operation the rather elaborate platinum thermometer arrangements for determining the exact temperature of the dielectric. A reference to the dielectric-temperature curve of rubidium hydrate† will show that even one or two degrees of temperature change in the neighbourhood of −185° C., or −200° pt. makes a very considerable alteration in the dielectric value. The result, however, ascertained is that changing the electromotive force from 1·434 volts to 17·8 or 79 volts does not bring down the dielectric constant from a large value to a small one.

In the same way the 5 per cent. solution of potassic hydrate was tested.

* 'Roy. Soc. Proc.,' vol. 61, p. 300, 1897.

† *Ibid.*, p. 378.

Using the 500-ohm galvanometer and an electromotive force of 9·88 volts we found 153 as the dielectric constant of the frozen electrolyte at the temperature of liquid air. Employing the 4-ohm galvanometer and 79·5 volts we found 175 as the dielectric constant.

All the above observations were taken at the temperature of liquid air and with an electromotive force frequency of 120. It is, therefore, quite clear that as far as these two frozen electrolytes are concerned, raising the charging voltage to a value far above that of the average electromotive force of polarisation does not bring down these abnormal values of the dielectric constant. On the other hand, a relatively small decrease in the temperature or *increase in the frequency at low temperatures* suffices to reduce the dielectric value of these frozen hydrates very considerably.* We may, therefore, say that the contention put forward by R. Abegg that the high dielectric values we have found for certain substances at the liquid air temperature are really polarisation capacities, does not seem to be borne out by the results of further experiment, and for the following reasons :—

(i) Because a very great increase in the charging electromotive force does not in any corresponding degree reduce the abnormally high dielectric values of certain frozen electrolytes to much smaller values.

(ii) Because when in the course of observations to construct a temperature dielectric curve, the working electromotive force has been changed from a value below the counter-electromotive force of polarisation to a value far above it, there is no break or discontinuity in the curve of dielectric value.

(iii) Because the great difference between quite similar electrolytes, such as the 10 per cent. solution of potassic and sodic carbonates, in respect of dielectric constant at equally low temperatures and under equal charging electromotive forces is left unexplained.

(iv) Because in the case of many substances, such as frozen ammonic hydrate, ice, and oxide of copper in suspension in ice, at very low temperatures, we find high dielectric values even though employing alternating currents of fairly high frequency (350 ∿).

* The effect of increased frequency of electromotive force reversals in decreasing the dielectric constant is evidently dependent upon the temperature as well as on the physical state of the body. In the case of water an increase in the frequency from zero to 10^6 hardly affects the dielectric constant at all. In the case of ice at 0°C. the same increase in frequency reduces the dielectric constant from 80 to between 2 and 3. In the case of ice at −50°C., as we have shown above, an increase in frequency from 120 to 350 reduces the dielectric constant from about 60 to about 3·6. (December 21, 1897.)

(v) Because the high dielectric values of water and alcohol and other bodies at ordinary temperatures remain, even when the observations are taken with alternating electromotive forces of exceedingly high frequency and under conditions when there are no electrodes to polarise, as when the electric refractive index is measured with electromagnetic radiation.

We consider that the results of observations so far made are best expressed by merely considering the dielectric constant to be a function of the frequency and the temperature, and represented therefore by a *dielectric surface*, which surface has for some substances a region of abnormal dielectric ordinate.

In all cases so far examined by us, lowering the temperature sufficiently acts in the same manner in reducing the dielectric constant as sufficiently increasing the frequency, and both actions reduce the abnormally large dielectric values of some substances to values more approximately equal to the square of the optical refractive index of the body.

The question then to be considered is the physical reason for the high dielectric values for particular substances for certain ranges of electromotive force frequency and temperature. Whether this abnormal electric displacement is considered to be the result of a molecular strain superimposed on a true electric strain, or whether it is the beginnings of that molecular deformation which finally ends in chemical decomposition, remains to be seen. Having regard to the enormously high electrical resistivity which we have shown these frozen dielectrics to possess, it does not appear to us likely that polarisation in the sense of a deposition of ions on the electrodes can be invoked to explain the differences we have shown exist.

December 16, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Professor Gabriel Lippmann was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "On a Method of determining the Reactions at the Points of Support of Continuous Beams." By GEORGE WILSON, M.Sc., Demonstrator in Engineering in the Whitworth Laboratory of the Owens College, Manchester. Communicated by Professor OSBORNE REYNOLDS, F.R.S.

II. "The Comparative Chemistry of the Suprarenal Capsules." By B. MOORE, M.A., Sharpey Research Scholar and Assistant in Physiology, University College, London, and SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar. Communicated by Professor SCHÄFER, F.R.S.

III. "Memoir on the Integration of Partial Differential Equations of the Second Order in Three Independent Variables, when an Intermediary Integral does not exist in general." By A. R. FORSYTH, F.R.S., Sadlerian Professor in the University of Cambridge.

IV. "On the Biology of *Stereum hirsutum*, Fr. By H. MARSHALL WARD, D.Sc., F.R.S., Professor of Botany in the University of Cambridge.

V. "An Examination into the Registered Speeds of American Trotting Horses, with Remarks on their value as Hereditary Data." By FRANCIS GALTON, D.C.L., F.R.S.

VI. "On the Thermal Conductivities of Single and Mixed Solids and Liquids, and their Variation with Temperature." By CHARLES H. LEES, D.Sc., Assistant Lecturer in Physics in the Owens College. Communicated by Professor SCHUSTER, F.R.S.

VII. "Cloudiness: Note on a Novel Case of Frequency." By KARL PEARSON, M.A., F.R.S., University College, London.

VIII. "On the Occlusion of Hydrogen and Oxygen by Palladium."
By LUDWIG MOND, Ph.D., F.R.S., WILLIAM RAMSAY, Ph.D.,
LL.D., Sc.D., F.R.S., and JOHN SHIELDS, D.Sc., Ph.D.

The Society adjourned over the Christmas Recess to Thursday,
January 20, 1898.

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"On a Method of determining the Reactions at the Points of
Support of Continuous Beams." By GEORGE WILSON,
M.Sc., Demonstrator in Engineering in the Whitworth
Laboratory of the Owens College, Manchester. Communi-
cated by Professor OSBORNE REYNOLDS, F.R.S. Received
November 20,—Read December 16, 1897.

The theory of continuous beams has been the subject of so much
research in the past that further investigation would seem almost
superfluous. In certain cases which occur in practice, however, the
computations arising out of the existing methods become com-
plicated and laborious, if not impossible to reduce, so that any
solution which avoids these difficulties may be of sufficient value to
warrant its publication.

Mr. Heppel, in a paper read before this Society,[*] has traced the
developments in the theory, culminating in the discovery of the
'Theorem of Three Moments,' by M. Bertot, in 1856, and independ-
ently by M.M. Clapeyron and Bresse, in 1857. Previously to
Clapeyron, Navier and other authors had sought the solution of the
problem by obtaining the reactions at the various points of support
of the beam; Clapeyron, however, first introduced the innovation of
considering the bending moments at the points of support as the
unknown quantities to be determined.

M. Bresse, in his 'Cours de Mécanique Appliquée,' has discussed
very fully the solutions of the various problems by this method, on
the supposition of a constant moment of inflexibility of the sections
of the beam both for the case of spans of arbitrary lengths and also
for cases where the end spars are equal but of different length to
the intermediate spans whose lengths are all supposed to be equal.

Mr. Heppel, in the above mentioned paper, published solutions in
which the spans were divided into two, three, four, or five equal
parts throughout each of which the load and the cross section of the
beam were supposed to remain constant, although varying from one
division to another.

Professors Perry and Ayrton[†] have dealt with the question of a

* 'Roy. Soc. Proc.,' vol. 18, p. 176.
† *Ibid.*, vol. 29, p. 493.

variable moment of inertia by obtaining the theorem of three moments in a slightly different form, the necessary summations for each span being performed graphically, whence on substitution in the original equations the bending moments can be obtained. The author has reverted to the problem of finding the reactions at the points of support and has based his method on a principle, definitely stated by Bresse,[*] and applied by him to the case of a uniform continuous beam of two equal spans.

The author claims that the method given below affords an easy and accurate solution for continuous beam problems, and especially those in which the moment of inertia is variable. It also permits of the variations in the stresses due to alterations in the levels of the supports being investigated.

The principle may be reproduced as far as is necessary as follows :—

The displacement of any point by reason of the deformation of the beam is the resultant of the displacements which would be produced if one supposed all the external known forces to act separately and one after the other.

This being so, the continuous beam may be considered as a simple beam supported at each end and under the action of the given loading acting vertically downwards, and also under the action of the supporting forces at the intermediate piers acting vertically upwards.

If the neutral fibre of the beam in the unloaded condition is assumed to be a straight line, then the result of the action of these two distinct systems of loading is to make the final deflection of the neutral fibre at each of the intermediate points of support equal to zero.

If the beam consist of n spans there will be $n-1$ intermediate supports, the upward pressure of each of which acting by itself would produce a definite deflection of the beam at any point: the sum of the separate deflections produced by these pressures at any one point of support must equal the deflection produced there by the given loading, and as each of the constituent deflections can be expressed in terms of the unknown concentrated load causing it, there will, therefore, be $(n-1)$ equations each containing the reactions at each intermediate point of support, and as there are $(n-1)$ reactions these equations are sufficient to determine their values.

Let $A_0 A_1 A_2 \ldots A_n$ be the points of support.

$$A_0 A_n = L.$$

$$A_0 A_1 = l_1, \qquad A_0 A_2 = l_2, \qquad A_0 A_3 = l_3 \ldots .$$

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[*] 'Cours de Mécanique Appliquée,' vol. 1, p. 137.

Let m be the bending moment at any point in the **mean fibre of** the beam, due to the given system of loading.

$I =$ the moment of inertia of the section of the beam upon which m acts.

$E =$ the modulus of elasticity.

Let the origin be at A_0; the axis of x be $A_0 A_1 \ldots A_n$; and the axis of y be perpendicular to that of x and positive downwards.

Also let the suffixes 0, 1, 2, 3,.... refer to the corresponding points A_0, A_1, A_2, A_3,.... so that m_1 is the bending moment at the first intermediate point of support, due to the given loading.

Then from the equation

$$E \frac{d^2 y}{dx^2} = -\frac{m}{I}$$

for the case of a beam supported at each end, we obtain

$$Ey = -\iint \frac{m}{I} dx^2 + ET_0 x,$$

where T_0 is the tangent of the angle of inclination to the axis of x, of the tangent to the mean fibre at the origin—

$$\therefore Ey_1 = -\iint_0^{l_1} \frac{m}{I} dx^2 + ET_0 l_1,$$

where y_1 is the deflection at A_1 which would be produced were the beam only supported at each end and under the action of the given loading.

Again let m', m'', m''', &c., be the bending moments at any point in the mean fibre due to the upward thrust of the reactions R_1, R_2, R_3.... at the points A_1, A_2, A_3...., and T_0', T_0'', T_0''' the corresponding tangents at the origin.

Then

$$Ey_1' = -\iint_0^{l_1} \frac{m'}{I} dx^2 + ET_0' l_1$$

$$Ey_1'' = -\iint_0^{l_1} \frac{m''}{I} dx^2 + ET_0'' l_1$$

hence finally since

$$y = y' + y'' + y''' + \ldots \text{ for the points } A_1, A_2, A_3 \ldots,$$

we have

$$ET_0 l_1 - \iint_0^{l_1} \frac{m}{I} dx^2 = \left(ET_0' l_1 - \iint_0^{l_1} \frac{m'}{I} dx^2\right) + \left(ET_0'' l_1 - \iint_0^{l_1} \frac{m''}{I} dx^2\right) + \ldots,$$

$$ET_0 l_2 - \iint_0^{l_2} \frac{m}{I} dx^2 = \left(ET_0' l_2 - \iint_0^{l_2} \frac{m'}{I} dx^2\right) + \left(ET_0'' l_2 - \iint_0^{l_2} \frac{m''}{I} dx^2\right) + \ldots,$$

and so on, there being as many equations as intermediate points of support.

If the function m/I is assumed to represent the intensity of a new loading on the girder, it can easily be shown that the expression

$$\mathrm{ET}_0 l_1 - \iint_0^{l_1} \frac{m}{\mathrm{I}} \, dx^2$$

represents the value of the bending moment at the point A_1, due to this new loading, considering the beam as supported only at each end.

Let this expression for the bending moment at A_1 be called N_1, and that at A_2 N_2, and similarly for $A_3 \ldots$.

Again, we may write the expression

$$\left(\mathrm{ET}_0' l_1 - \iint_0^{l_1} \frac{m'}{\mathrm{I}} \, dx^2 \right) \text{ equal to } R_1 n_1',$$

n_1' being the bending moment at A_1, due to a new "m/I" loading obtained by assuming a unit force to act at A_1.

Similarly $\qquad \left(\mathrm{ET}_0'' l_1 - \iint_0^{l_1} \frac{m''}{\mathrm{I}} \, dx^2 \right) = R_2 n_1''.$

Hence the equations become

$$N_1 = R_1 n_1' + R_2 n_1'' + R_3 n_1''' + \ldots,$$

$$N_2 = R_1 n_2' + R_2 n_2'' + R_3 n_2''' + \ldots,$$

$$N_3 = R_1 n_3' + R_2 n_3'' + R_3 n_3''' + \ldots,$$

$$\&c., \&c.,$$

from which $R_1, R_2, R_3 \ldots$ may be easily obtained when the constants have been determined.

Lord Rayleigh has shown, in his 'Theory of Sound' (vol. 1, p. 69), that when a beam is loaded with a concentrated load at any point P_1, and the load is transferred to a second point P_2, then the deflection at P_2 when the load is at P_1 is equal to the deflection at P_1 when the load is at P_2, hence

$$n_2' = n_1''; \; n_3' = n_1'''; \; n_3'' = n_2''; \; \&c., \&c.,$$

thus reducing the number of constants to be found, or affording a check on the accuracy of the working.

In the example appended

$$n_2' = 22\cdot47 \text{ by calculation.}$$

$$n_1'' = 22\cdot53 \quad\quad \text{,,}$$

$$\text{Mean} = 22\cdot50$$

$$\text{Error} = \frac{}{4} \quad 0\cdot13 \text{ per cent.}$$

In practice it is usually very difficult to obtain the value of the $\iint_0 \frac{m}{I} dx^2$ by integration unless I is assumed to be constant, or some simple function of x.

The following method will, however, give the values of N, n', n''.... required to any degree of accuracy.

FIG. I.

Let the diagram APCA (fig. 1), obtained by erecting ordinates proportional to the values of m/I at every point in the mean fibre AC, represent the new loading.

The bending moment at any point B may be found very simply as follows :—

If PN be any ordinate such that PN·dx represents the load on any small element dx at N, then if PN be divided in Q so that QN/PQ = NC/NA, then QNdx represents that part of the reaction at A due to this load PNdx at N.

Hence by taking a sufficient number of points a curve AQC can be drawn to represent the reaction at A due to the load APC. Again, the load on AB may be replaced by a load at A having an equal moment about B, in a similar manner, either graphically as in the figure, which explains itself, or by calculation. Thus a second curve, ARB, is obtained.

Then it is easily seen that the moment at B is the difference of the areas of the diagrams AQCA and ARBA, multiplied by AB, that is—

$$N_1 = AB(\text{area AQCBRA}).$$

This area may be obtained by a planimeter or by calculation, for

if all the ordinates to the curves are taken at equal intervals, the span being divided into an even number of the latter, the final diagram AQCBRA may be plotted and its area obtained by any planimeter, or by Simpson's rule.

In the example appended the area has been found both ways.

Elevation or Depression of Points of Support.

Let δ with the correct suffix represent the elevation of any point of support above the line $A_0 A_n$, a depression being reckoned as negative.

Then in the equations to find the reactions, since the final deflection is not zero, we must write

$$N_1 + E\hat{c}_1 = R_1 n_1' + R_2 n_1'' + \dots .$$
$$N_2 + E\hat{c}_2 = R_1 n_2' + R_2 n_2'' + \dots .$$

Some of the values of $R_1 R_2 \dots$ may be negative, in which case, if the beam is not to be fastened down at the supports, a fresh solution must be sought by omitting one or more of the negative reactions, until the remaining ones become positive.

The mean fibre of the beam has hitherto been assumed a straight line when under the action of no force. In certain girders this is not the case, but the above methods may be applied with sufficient accuracy for practical purposes when the maximum distance of the external layer of the beam from the mean fibre is small compared with the original radius of curvature of the mean fibre.

For then $\qquad \dfrac{m}{EI} = \left(\dfrac{1}{R'} - \dfrac{1}{R} \right)$ approximately,

where R is the original radius of curvature of the mean fibre at any point and R' its curvature after loading.

Hence, if $v = F(x)$ is the original equation to the mean fibre, and $y = f(x)$ the equation after straining,

then $\qquad\qquad \dfrac{m}{EI} = \dfrac{d^2 y}{dx^2} - \dfrac{d^2 v}{dx^2}$

or $\qquad\qquad \dfrac{d^2 y}{dx^2} = \dfrac{d^2 v}{dx^2} + \dfrac{m}{EI} ,$

which shows that the final deflection curve is the result of superposing on the original curve of the mean fibre, the deflection curve obtained under the given loading, for a beam of the same cross

section at every point, but with a straight mean fibre, and hence the method is applicable.

Appendix.

To find the magnitude of the reactions at the points of support of a continuous girder of three spans (viz., 60 feet, 100 feet, and 40 feet respectively), and loaded with a uniform load of 3 tons per foot run, the moment of inertia of the cross section being 2100 at the commencement of the 60-foot span and increasing uniformly to 3300 at the commencement of the 100-foot span, and thence diminishing uniformly to 3000 at the beginning of the 40-foot span, and further diminishing uniformly to 2200 at the other end of the girder, the units taken being feet and tons.

The variation of the moment of inertia is shown in fig. 2. The curve A_0BCA_3 in fig. 3 is the M/I curve for the beam resting on each end support, and loaded with 3 tons per foot run.

Treating this as a new load and reducing the ordinates in the correct proportion, the curve A_0DEA_3 shows the amount of this load transmitted to A_0 to form the reaction there.

FIC.2.
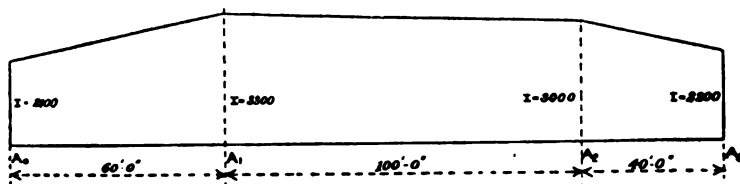
FIC.3.
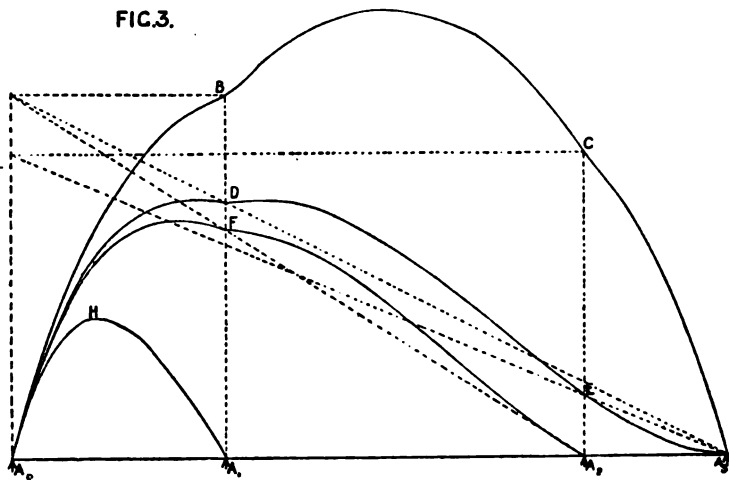
Again the curves A_0HA_1 and A_0FA_2 show the loads acting at A_0, which have their moments about A_1 and A_2 respectively, equal to the moments of the loads on A_0A_1 and A_0A_2 about A_1 and A_2 respectively.

Hence $\quad$ $N_1 = $ area $A_0DEA_3A_1HA_0 \times\ 60 = 16330.$

$\qquad\qquad N_2 = $ area $A_0DEA_3A_2FA_0 \times 160 = 12084.$

Similarly the curve A_0BCA_3 in fig. 4 is the M/I curves for 100-ton load acting at A_1, whence

$$n_1' = \text{area } A_0DEA_3A_1HA_0 \times \frac{60}{100} = 37\cdot67.$$

$$n_2' = \text{area } A_0DEA_3A_2FA_0 \times \frac{160}{100} = 22\cdot47.$$
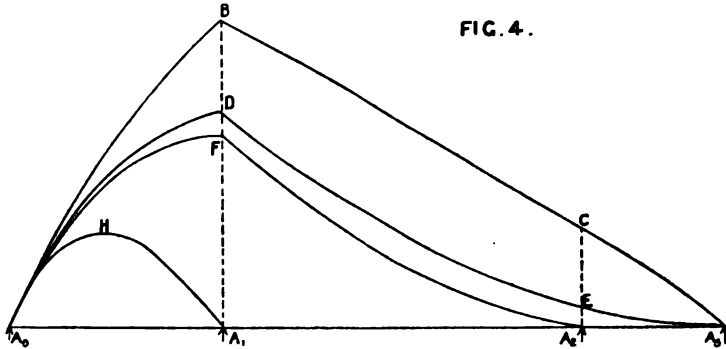
FIG. 4.

Again the curve A_0BCA_3 in fig. 5 shows the M/I curve for 100 tons load acting at A_2.

whence $\quad$ $n_1'' = $ area $A_0DEA_3A.HA_0 \times \dfrac{60}{100} = 22\cdot53.$

$\qquad\qquad n_2'' = $ area $A_0DEA.A_2FA_0 \times \dfrac{60}{100} = 22\cdot69.$
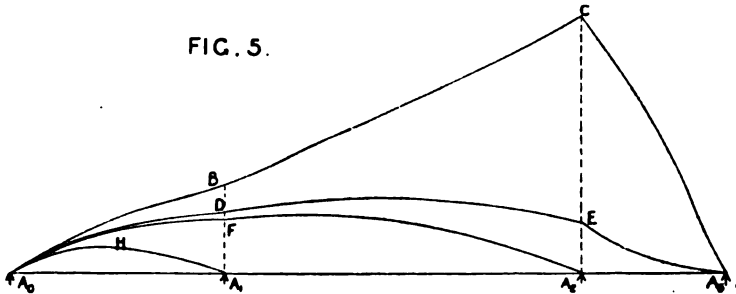
FIG. 5.

The equations for the supports are

$$16330 = 37\cdot67\ R_1 + 22\cdot53\ R_2,$$
$$12034 = 22\cdot47\ R_1 + 22\cdot69\ R_2,$$

whence

$$R_0 = 52\cdot0 \text{ tons.}$$
$$R_1 = 281\cdot9 \quad \text{,,}$$
$$R_2 = 253\cdot5 \quad \text{,,}$$
$$R_3 = 12\cdot6 \quad \text{,,}$$

If I were assumed to be constant then by Clapeyron's theorem of three moments

$$R_0 = 53\cdot2 \text{ tons.}$$
$$R_1 = 278\cdot3 \quad \text{,,}$$
$$R_2 = 259\cdot8 \quad \text{,,}$$
$$R_3 = 8\cdot7 \quad \text{,,}$$

The above values of the constants were obtained by dividing the span into twenty equal divisions of 10 feet, and calculating the value of the ordinates to each curve at these points. This is clearly shown in Tables I, II, III.

Having obtained the ordinates to the M/I curve, in each case the succeeding ordinates could be written down almost by inspection, and employed to find the areas by Simpson's rule, the time occupied being very short.

If the support at A_1 be supposed to be depressed $\frac{1}{2}$ inch below the line $A_0 A_3$ whilst A_2 is supposed to be elevated $\frac{3}{4}$ inch above it, the alteration in the supporting forces is easily found.

For assuming E = 24,000,000 lbs.

$$E\hat{c}_1 = -446\cdot4.$$
$$E\hat{c}_2 = +669\cdot6.$$

Hence the equations become

$$16330 - 446\cdot4 = 37\cdot67\ R_1 + 22\cdot53\ R_2,$$
$$12084 + 669\cdot6 = 22\cdot47\ R_1 + 22\cdot69\ R_2,$$

whence

$$R_0 = 82\cdot06.$$
$$R_1 = 210\cdot2.$$
$$R_2 = 354\cdot0.$$
$$R_3 = -46\cdot26,$$

indicating that under these conditions the end of the girder at A_3 would have to be fastened down in some way.

Further alterations of level may be investigated in a similar manner.

Table I.

Distance of ordinate from A_0 (feet)	0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
Moment of inertia (feet units)	2100	2300	2500	2700	2900	3100	3300	3270	3210	3210	3180	3150	3120	3000	3060	3030	3000	2800	2600	2400	2200
Bending moment in foot tons	0	2850	5100	7650	9600	11250	12600	13650	14400	14850	15000	14850	14400	13350	12800	11250	9600	7650	5400	2850	0
Ordinates to m/I curve	0	1·239	2·160	2·853	3·310	3·629	3·818	4·174	4·444	4·696	4·717	4·714	4·615	4·418	4·117	3·712	3·290	2·732	2·077	1·187	0
Reaction at A_0 due to m/I loading	0	1·177	1·944	2·408	2·648	2·722	2·673	2·713	2·066	2·544	2·355	2·121	1·846	1·546	1·295	0·925	0·640	0·410	0·208	0·060	0·000
Load at A_0 having moment about A_1, equivalent to that of m/I loading on A_0A_1	0	1·033	1·440	1·416	1·703	0·805	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Differences = ordinates to area $A_0DEA_1A_1HA_0$ in fig. 3	0	0·144	0·504	0·692	1·545	2·117	2·673	2·713	2·696	2·544	2·358	2·121	1·846	1·546	1·295	0·925	0·640	0·410	0·208	0·060	0·000
Hence area by Simpson's rule =	272·166							$N_1 = 272·166 \times 60 = 16330.$													
Load at A_0 due to m/I loading	0	1·177	1·944	2·408	2·648	2·722	2·673	2·713	2·066	2·544	2·355	2·121	1·846	1·546	1·255	0·928	0·640	0·410	0·205	0·000	0
Load at A_0 having moment about A_2, equivalent to that of m/I loading on A_0A_2	0	1·169	1·850	2·302	2·483	2·495	2·386	2·348	2·222	2·024	1·769	1·473	1·154	0·888	0·515	0·232	0	0	0	0	0
Differences = ordinates to area $A_0DEA_2FA_0$ in fig. 3	0	0·015	0·054	0·106	0·165	0·227	0·287	0·365	0·444	0·520	0·589	0·648	0·692	0·718	0·720	0·696	0·640	0·410	0·208	0·060	0
Hence area by Simpson's rule =	75·53							$N_2 = 75·53 \times 160 = 12084.$													

Table II.

Distance of ordinate from A₀ (feet)	0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
Moment of inertia (feet units)	2100	2300	2500	2700	2900	3100	3300	3210	3240	3210	3180	3150	3126	3080	3050	3030	3000	2800	2600	2100	2200
Bending moment (ft. tons) due to 100 tons at A_1	0	700	1400	2100	2800	3500	4200	3800	3800	3300	3000	2700	2400	2100	1800	1500	1200	900	600	300	0
Ordinates to m/I curve	0	0·304	0·560	0·778	0·966	1·129	1·272	1·192	1·112	1·028	0·944	0·857	0·769	0·680	0·558	0·495	0·400	0·321	0·231	0·125	0
Reaction at A_0 due to m/I loading	0	0·289	0·504	0·661	0·773	0·847	0·890	0·775	0·667	0·565	0·472	0·386	0·308	0·238	0·177	0·124	0·080	0·048	0·023	0·005	0
Load at A_0 having moment about A_1, equivalent to that of m/I loading on A_0A_1	0	0·253	0·373	0·389	0·322	0·188	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Differences = ordinates to area $A_0DBA_3A_1HA_0$ in fig. 4	0	0·131	0·272	0·451	0·659	0·890	0·775	0·667	0·565	0·472	0·386	0·308	0·238	0·177	0·124	0·080	0·048	0·023	0·005	0	

Hence area by Simpson's rule = 62·783 $\therefore w'_1 = \dfrac{62 \cdot 783 \times 60}{100} = 37 \cdot 67.$

	0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
Load at A_0 due to m/I loading	0	0·289	0·504	0·661	0·773	0·847	0·890	0·775	0·667	0·565	0·472	0·386	0·308	0·238	0·177	0·124	0·080	0·048	0·023	0·005	0
Load at A_0 having moment about A_2 equivalent to that of m/I loading on A_0A_2	0	0·285	0·499	0·632	0·724	0·776	0·795	0·671	0·556	0·450	0·354	0·268	0·192	0·127	0·074	0·031	0	0	0	0	0
Differences = ordinates to area $A_0DBA_3A_2FA_0$ in fig. 4	0	0·004	0·029	0·049	0·071	0·065	0·104	0·111	0·115	0·118	0·118	0·116	0·111	0·103	0·093	0·080	0·048	0·023	0·005	0	

Hence area by Simpson's rule = 14·046 $\therefore w'_2 = \dfrac{14 \cdot 046 \times 160}{100} = 22 \cdot 47.$

Table III.

Distance of ordinates from A₀ (feet)	0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
Moment of inertia (feet units)	2100	2300	2500	2700	2800	3100	3300	3270	3240	3210	3150	3150	3120	3090	3060	3030	3000	2800	2600	2400	2200
Bending moment due to 100 tons at A_2	0	200	400	600	800	1000	1200	1400	1600	1800	2000	2200	2400	2600	2800	3000	3200	2400	1600	800	0
Ordinates to m/I curve	0	0·087	0·160	0·222	0·278	0·323	0·364	0·428	0·494	0·561	0·629	0·698	0·769	0·841	0·915	0·990	1·067	0·857	0·615	0·333	0
Reaction at A_0 due to m/I loading	0	0·063	0·144	0·189	0·221	0·242	0·255	0·278	0·296	0·309	0·315	0·315	0·308	0·294	0·275	0·245	0·213	0·134	0·062	0·017	0
Load at A_0 having moment about A_1, equivalent to that of m/I loading on A_0A_1	0	0·073	0·107	0·111	0·092	0·054	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Differences = ordinates to area $A_0DEA_1HA_0$ in fig. 5	0	0·010	0·037	0·078	0·129	0·188	0·255	0·278	0·296	0·309	0·315	0·315	0·308	0·294	0·275	0·245	0·213	0·134	0·062	0·017	0

Hence area by Simpson's rule $= 37\cdot55$ $\therefore$ $n_1'' = \dfrac{37\cdot55 \times 60}{100} = 22\cdot53.$

	0	10	20	30	40	50	60	70	80	90	100	110	120	130	140	150	160	170	180	190	200
Load at A_0 due to m/I loading	0	0·063	0·144	0·189	0·221	0·242	0·255	0·278	0·296	0·309	0·315	0·315	0·308	0·294	0·275	0·248	0·213	0·134	0·062	0·017	0
Load at A_0 having moment about A_2, equivalent to that of m/I loading on A_0A_3	0	0·062	0·140	0·180	0·207	0·222	0·226	0·241	0·247	0·245	0·236	0·218	0·192	0·158	0·114	0·062	0	0	0	0	0
Differences = ordinates to area $A_0DEA_3A_2FA_0$ in fig. 5	0	0·001	0·004	0·009	0·014	0·020	0·027	0·087	0·049	0·064	0·079	0·097	0·116	0·136	0·161	0·186	0·213	0·134	0·062	0·017	0

Hence area by Simpson's rule $= 14\cdot18$ $\therefore$ $n_2'' = \dfrac{14\cdot18 \times 100}{100} = 22\cdot69.$

The Comparative Chemistry of the Suprarenal Capsules." By
B. MOORE, M.A., Sharpey Research Scholar and Assistant
in Physiology, University College, London, and SWALE
VINCENT, M.B. (Lond.), British Medical Association Research
Scholar.   Communicated by Professor SCHÄFER, F.R.S.
Received November 23,—Read December 16, 1897.

(From the Physiological Laboratory of University College, London.)

It has been fairly well established that the paired suprarenal bodies
in connection with the sympathetic nervous system of Elasmobranch
fishes correspond structurally and functionally to the medulla of
mammalian suprarenal capsules.* There is also considerable evidence
in favour of the homology of the inter-renal body of Elasmobranchs
(and of the suprarenal bodies of Teleosts and Ganoids) with the
cortex of mammalian suprarenals.   This evidence in the case of the
cortical glands is chiefly morphological and histological, the experi-
mental results being purely negative.*

The paired suprarenal bodies of Elasmobranchs have been shown
to produce the characteristic rise of blood-pressure when an extract
of them is injected intravenously into a living mammal, or persistent
contraction of arterioles when an extract is perfused through the
blood-vessels of a pithed frog.† It has further been demonstrated that
a subcutaneous injection of an extract from these same bodies will
produce fatal results in mice, just in the same way as if prepared from
the medulla of mammalian supra-renals.‡ The inter-renal of Elasmo-
branchs and the known suprarenals of Teleosts have no physiological
action either administered intravenously or subcutaneously,†‡ thus
rendering it probable that they consist of cortical material only, as
had appeared from their histology.*

Up to the present the comparative chemistry of the subject has not
been investigated.

It has been shown that the medulla of mammalian suprarenal con-
tains a substance exhibiting well-marked colour reactions.§ Like
the physiologically active substance of the medulla, this chromogen
has not yet been isolated, but it always occurs closely associated with

* Collinge and Vincent, 'Anat. Anz.,' vol. 12, Nos. 9 and 10, 1896; Vincent,
'Birm. Nat. Hist. and Phil. Soc. Proc.,' 1896, vol. 10, Part I ; and ' Zool. Soc. Lond.
Trans.,' vol, 14, Part III, 1897.

† Swale Vincent, ' Anat. Anz.,' vol. 13, Nos. 1 and 2, 1897, and ' Roy. Soc. Proc.,'
vol. 61, 1897, p. 64; also ' Physiol. Soc. Proc.,' Mar. 20, 1897.

‡ ' Roy. Soc. Proc. (in the press).

§ Vulpian, ' Compt. Rend.,' vol. 43, 1856; *ibid.*, vol. 44, 1857; Virchow, in
' Virchow's Archiv.,' vol. 12, 1857 ; Arnold, *ibid.*, vol. 35, 1866 ; Krukenberg, *ibid.*,
vol. 101, 1885.

the active material.* When active suprarenal material is subjected to the action of strong alcohol (98 per cent.) for seven to ten days, the active material becomes destroyed, as shown by the physiological test of intravenous injection, but the chromogen is unaffected, and continues to give the usual colour tests in a characteristic fashion.† Hence the two substances are not identical, but their close association suggests the probability that the active material has a very complex molecule (for only such a molecule would be destroyed by prolonged contact with alcohol), and that the group giving the colour reactions forms an integral part of this molecule, and is not broken up in the decomposition.‡

The colour reactions referred to above are common to all ortho-dihydroxy-benzene derivatives, and are briefly as follows :—

1. Addition of various oxidising agents causes a rose-red coloration. This can be produced by addition of bromine-, or iodine-water, or solution of hydrogen peroxide, or alkalis.§

2. Addition of ferric chloride to a neutral solution causes a deep green colour.

3. Certain metallic salts are precipitated and then reduced, *e.g.*, addition of $AgNO_3$ causes a white precipitate which rapidly becomes black from reduction, especially on warming. Similarly, phospho-molybdic acid produces a yellowish precipitate, which, as well as the yellow solution, rapidly turns green.

4. Addition of potassium chromate causes a deep brown colour, probably due to an admixture of the colour of the chromate with the products of oxidation of the chromogen.

Although there is no purely chemical test known which indicates the presence of the physiologically active material, these colour tests make it easy to demonstrate with certainty the presence of the chromogen. Now, as the chromogen and the active material are closely associated in the mammalian medulla, and, as it has further been shown that the paired bodies of Elasmobranchs contain the active material,‖ it appeared to us of importance to determine whether the chromogen was also present in these structures. Accordingly, the following experiment was performed.

* Moore, ' Physiol. Soc. Proc.,' March, 1894 (' Journ. of Physiol.,' vol. 17, 1895, p. 14).

† This applies not only to mammalian medulla but also to the medullary suprarenals of Elasmobranch fishes (*vide* Vincent, ' Roy. Soc. Proc.' vol. 61, 1897, p. 64).

‡ Moore, ' Journ. of Physiol.,' vol. 21, 1897, p. 382.

§ This rose-red colour produced by alkalis immediately and completely disappears on making acid again, and once more returns on making alkaline; the change in colour may be repeated as often as is desired. It is also intensified if the suprarenal extract has previously been boiled in acid solution.

‖ Vincent, *loc. cit.*

Thirteen perfectly fresh, medium-sized specimens of *Scyllium canicula* were dissected, and the paired suprarenals and the inter-renals carefully removed. The paired bodies were all picked out first, and then knife and forceps were changed for the removal of the inter-renal, so as to avoid the possibility of contamination by their means.

The paired suprarenal bodies were found to weigh in a moist state 0·7 gram, while the inter-renals obtained amounted to 0·33 gram.

Each of these was boiled in water for a short time, and allowed to stand for about a quarter of an hour to allow time for complete extraction. The decoctions were made up to a strength of 10 per cent. of the moist glands. After filtering, the extract obtained from the paired suprarenals was of a pale brownish-pink colour with a distinct fluorescence, while that from the inter-renals was much paler, being light yellow and devoid of fluorescence.

On testing these two solutions, it was found that :—

1. Ferric chloride gave a deep green coloration with the decoction from the paired suprarenals, while no change was produced in the case of the inter-renal extract.

2. Iodine water, when added to the solution obtained from the paired bodies, produced a decided pink colour, while it effected no change in the tint of the inter-renal fluid.

3. Hydrogen peroxide gave a pink coloration in the case of the paired bodies only.

4. Caustic potash gave with the paired suprarenal extract an immediate dirty brown colour, but if a drop of weak hydrochloric acid had been previously added, a pink coloration immediately ensued. This reagent gave no effect in the case of the inter-renal.

5. Potassium chromate produced a deep brown coloration with the decoction obtained from the paired bodies,* but gave no change in that from the inter-renal gland.

6. Silver nitrate gave a white precipitate which immediately became black with the decoction of the paired bodies, but no effect with that of the inter-renals.

7. Phospho-molybdic acid gave, in the case of the paired bodies, a yellow precipitate which, as well as the solution, turned green from reduction, gradually in the cold, more rapidly on warming. This reaction was not obtained in the case of the inter-renals.

These reactions prove conclusively that a chromogen having the same chemical nature as that found in mammalian medulla is found in the paired segmental suprarenal bodies of Elasmobranch fishes.

* This deep brown colour with salts of chromic acid has been employed to display the medullary glands for purposes of dissection. It is a convenient means of picking out medulla from cortex in those animals in which the two are united into one organ.

It has been concluded from histological and physiological evidence that the suprarenal bodies of Teleostean fishes consist solely of cortex.* The physiologically active material is wanting, as in cortical substance elsewhere, and it would be interesting to determine the presence or absence of the chromogen. Unfortunately we have been unable so far to obtain sufficient material for chemical examination. The same remarks apply to the suprarenal bodies of the Ganoids.

For the purpose of comparison, we have chemically tested an extract made from the suprarenal glands of the frog. Six good-sized animals were killed and the suprarenals snipped off from the kidney with scissors. Although there was a considerable admixture of kidney substance with the material thus obtained, the weight in a moist state only amounted to 0·13 gram. This was treated in the way described above for the Elasmobranch material, and gave the chromogen reactions in a perfectly definite manner.

"Memoir on the Integration of Partial Differential Equations of the Second Order in Three Independent Variables, when an Intermediary Integral does not exist in general." By A. R. FORSYTH, F.R.S., Sadlerian Professor in the University of Cambridge. Received November 23,— Read December 16, 1897.

(Abstract.)

The general feature of most of the methods of integration of any partial differential equation is the construction of an appropriate subsidiary system and the establishment of the proper relations between integrals of this system and the solution of the original equation. Methods, which in this sense may be called complete, are possessed for partial differential equations of the first order in one dependent variable and any number of independent variables; for certain classes of equations of the first order in two independent variables and a number of dependent variables; and for equations of the second (and higher) orders in one dependent and two independent variables. The present memoir discusses the theory of partial differential equations of the second order in one dependent and three independent variables; and the method adopted is seen, without difficulty, to be applicable to equations which involve more than three independent variables and which can be of order higher than the second. The reason why equations of the type considered are taken to be such as do not possess an intermediary integral, that is, a

* Vincent, *loc. cit.*

differential equation of lower order in virtue of which the original equation is satisfied, is that the other equations, which do possess an intermediary integral, are a class apart and have been considered elsewhere.*

In order to solve a given equation, a system of subsidiary equations is constructed; and the system is made up of two parts. One of these parts is a set of simultaneous partial differential equations in two independent variables and a number of dependent variables, this number being one more than the number of the equations. An integral equivalent of this part accordingly contains an undetermined quantity. The other of the parts is a set of equations in a single independent variable; it appears that the set of equations in the second part can be consistently satisfied by a determination of the unknown quantity emerging from the first part.

In particular, the equations represented by

$$F(a, b, c, f, g, h, l, m, n, v, x, y, z) = 0,$$

where x, y, z are the independent variables, v is the dependent variable, l, m, n are its first derivatives, and a, b, c, f, g, h are its second derivatives, are found to divide themselves into two distinct classes according as the equation

$$\xi^2\frac{\partial F}{\partial a} + \eta^2\frac{\partial F}{\partial b} \mid \zeta^2\frac{\partial F}{\partial c} + 2\xi\eta\frac{\partial F}{\partial h} + 2\eta\zeta\frac{\partial F}{\partial g} + 2\eta\zeta\frac{\partial F}{\partial f} = 0$$

is resolvable or is not resolvable into two equations linear in ξ, η, ζ. When this equation, called the characteristic invariant on account of an invariantive property which it is proved to possess, is resolvable into two linear equations, the process of integration of the subsidiary equations is much simplified.

The first of the three sections, into which the paper is divided, deals with the general theory, and indicates a method whereby subsidiary equations for an equation $F = 0$ of any degree in the derivatives of the second order can be constructed. If integrable combinations of the subsidiary system are not obtainable, an extension of the method shows how equations of higher order (when obtainable) can be deduced and associated with the given equation.

The second of the three sections deals with those equations of which the characteristic invariant is resolvable; and some examples are given, alike of equations for which the integration of the initial subsidiary system is possible, and of equations for which the extended method must be used.

The third of the three sections deals with those equations of which the characteristic invariant is irresolvable. Of such equations the

* 'Camb. Phil. Trans.,' vol. 16, pp. 191-218.

most interesting examples (from the point of view of application to mathematical physics) are the equations

$$\nabla^2 v = 0,$$
$$\nabla^2 v = -\kappa^2 v,$$

or, with the notation of the memoir,

$$a + b + c = 0,$$
$$a + b + c = -\kappa^2 v;$$

and the theory is applied to these equations in detail. Solutions, which are believed to be new, are obtained for both of them; each solution involves two explicit arbitrary functional forms, and the argument of each of these arbitrary functions itself involves an arbitrary element; but in each case the solution is not that of the widest possible generality which the equation is known to possess. To quote one result: a solution of the equation

$$a + b + c = 0$$

can be stated as follows :—

Let p, q, r denote three arbitrary functions of u subject solely to the condition

$$p^2 + q^2 + r^2 = 0;$$

let u be determined as a function of x, y, z by means of the equation

$$au = xp(u) + yq(u) + zr(u),$$

where a is a constant, and let v denote

$$H(u) + \frac{G(u)}{a - xp'(u) - yq'(u) - zr'(u)},$$

where G and H are distinct arbitrary functions: then v satisfies the equation

$$a + b + c = \nabla^2 v = 0.$$

"On the Biology of *Stereum hirsutum*, Fr." By H. MARSHALL WARD, D.Sc., F.R.S., Professor of Botany in the University of Cambridge. Received November 23,—Read December 16, 1897.

(Abstract.)

The author has cultivated the mycelium of this fungus obtained from spores, on sterilised wood, and after several months the cultures developed yellow bosses which proved to be the hymenophores bearing the basidia. This fungus has not hitherto been made to produce

spores in cultures, and Basidiomycetes generally have rarely been made to do so.

The actions of the mycelium on the wood of *Æsculus, Pinus, Quercus,* and *Salix* are also examined, and this is, so far as known, the first time this has been done with pure cultures.

Anatomical and histological details, with figures, are given in the complete paper.

" On the Thermal Conductivities of Single and Mixed Solids and Liquids, and their Variation with Temperature." By CHARLES H. LEES, D.Sc., Assistant Lecturer in Physics in the Owens College. Communicated by Professor SCHUSTER, F.R.S. Received November 30,—Read December 16, 1897.

(Abstract.)

These experiments were undertaken with a view to determining the effect of temperature on thermal conductivities, and the relation between the conductivity of a mixture and the conductivities of its constituents. The apparatus consisted of a number of flat circular copper discs, into each of which a thermo-junction was soldered. The substances to be experimented on were placed between these discs, heat was supplied to one of the discs at a measured rate, by passing an electric current through a coil in contact with it, and the differences of temperature between the discs were measured by balancing the thermo-electromotive forces produced, against the fall of potential down a wire. About thirty solids, liquids, substances near their melting points, and mixtures of liquids, were tested between temperatures of 15° and 50° C., and the following statements embody the results :—

1. Solids not very good conductors of heat in general decrease in conductivity with increase of temperature in the neighbourhood of 40° C. Glass is an exception to this rule.
2. Liquids follow the same law in the neighbourhood of 30° C.
3. The conductivity of a substance does not invariably change abruptly at the melting point.
4. The thermal conductivity of a mixture lies between the conductivities of its constituents, but is not a linear function of its composition.
5. Mixtures of liquids decrease in conductivity with increase of temperature in the neighbourhood of 30° C., at about the same rate as their constituents.

" Cloudiness: Note on a Novel Case of Frequency." By KARL
PEARSON, M.A., F.R.S., University College, London. Re-
ceived December 1,—Read December 16, 1897.

In a memoir on Skew Variation, contributed some time back to
the 'Philosophical Transactions,'[*] I pointed out (p. 364) that we
might expect theoretically to occasionally find ∪-shaped distribu-
tions of frequency. I was unable at that time to refer to any case
actually known to me except Mr. Francis Galton's curve of "con-
sumptivity." The data in that case did not seem to me sufficiently
definite to base any elaborate calculations upon them. Quite recently,
in studying Hugo Meyer's 'Anleitung zur Bearbeitung meteorolo-
gischer Beobachtungen für die Klimatologie,' Berlin, 1891, I came
across, on S. 108, the table for the frequency of various degrees of
cloudiness for the decade 1876–85, at Breslau. Although the
method used for determining the extent of cloudiness is not entirely
satisfactory, and, as Herr Meyer remarks, the observer must have had
some personal bias with regard to the grade 9, still the observations
are so numerous, and so markedly ∪-shaped, that I thought it well
worth investigating how far my theory of skew variation would
suffice to describe such a novel form of frequency.

The observations are as follows :—

Degrees of Cloudiness at Breslau, 1876—1885.

Degree	0	1	2	3	4	5	6	7	8	9	10
Frequency....	751	179	107	69	46	9	21	71	194	117	2089

The total number of days of observation is 3,653.

Clearly no cloudiness and absolute cloudiness are both maxima
while the mean cloudiness will not be very far removed from mini-
mum frequency.

The following data were obtained for the distribution by Miss
Alice Lee, by the methods of the memoir referred to above :—

$$\text{Mean} = 6\cdot8292 \qquad\qquad \beta_1 = 0\cdot6112$$

$$\mu_2 = 18\cdot2999 \qquad\qquad \beta_2 = 1\cdot7414$$

$$\mu_3 = -61\cdot2030 \qquad 6 + 3\beta_1 - 2\beta_2 = 4\cdot3508$$

$$\mu_4 = 583\cdot1838$$

The theoretical curve is thus one of limited range.

[*] Series A, vol. 186, 1895.

Proceeding we found

$$\epsilon = 0.00699 \qquad\qquad r = 0.17958$$

$$m_1 = -0.8774 \qquad\qquad a_1 = 4.8109$$

$$m_2 = -0.9430 \qquad\qquad a_2 = 5.1705$$

The negative values of m_1 and m_2 show us that the theoretical curve has changed from its usual form to a $\bigcup$-shaped figure. The range given is $b = a_1 + a_2 = 9.9814$, instead of the actual 10.

The distance d between mean and mode

$$= \frac{a_2 - a_1}{r} = 2.0022.$$

Thus the start of the range is $4.8270 - 4.8109 = 0.016$, instead of 0, and it runs to 9.998, instead of 10. We conclude accordingly that if the range of possible cloudiness had been quite unknown *à priori*, it would have been closely given by theory.

The modal frequency y_0 was found to be 50.7505.

Thus the theoretical equation to the frequency is—

$$y = 50.7505 \left(1 + \frac{x}{4.8109}\right)^{-0.8774} \left(1 - \frac{x}{5.1705}\right)^{-0.9430},$$

the origin being at 4.8270.

The modal value now corresponding to a minimum and not to a maximum as usual, the name "mode" ceases to be appropriate.[*] The observations and the above curve are given in the accompanying diagram, and it will be seen that there is a complete transformation of the usual frequency distribution to fit the altered state of affairs. With the asymptotic character of the curve, it is impossible to compare ordinates as giving the frequencies between 0 and 1, and 9 and 10. Accordingly the areas of the curve between 0.016 and 0.5, and between 9.5 and 9.998 were taken as the true measure of the frequencies of the degrees 0 and 10. These were obtained by means of the following formulæ :—

$$A_1 = (y_0 b) \times \frac{n_1{}^{n_1} n_2{}^{n_2}}{(n_1 + n_2)^{n_1 + n_2}} \times \left(\frac{x_1}{b}\right)^{1-n_1} \left\{ \frac{1}{1-n_1} + \frac{n_2}{2-n_1}\left(\frac{x_1}{b}\right) + \frac{n_2(n_2+1)}{2(3-n_1)}\left(\frac{x_1}{b}\right)^2 \right\},$$

$$A_2 = (y_0 b) \times \frac{n_1{}^{n_1} n_2{}^{n_2}}{(n_1 + n_2)^{n_1 + n_2}} \times \left(\frac{x_2}{b}\right)^{1-n_2} \left\{ \frac{1}{1-n_2} + \frac{n_1}{2-n_2}\left(\frac{x_2}{b}\right) + \frac{n_1(n_1+1)}{2(3-n_1)}\left(\frac{x_2}{b}\right)^2 \right\},$$

where

$$n_1 = -m_1, \qquad\qquad n_2 = -m_2,$$

[*] The name *antimode* is now convenient.

and A_1 and A_2 are respectively the areas of the curve measured for small lengths x_1 and x_2 at either end of the range.

The following gives a comparison of the frequencies of the various degrees of cloudiness, as given by observation, and by the areas of the curve :—

Degree.	Observation.	Calculation.
0	751	803
1	179	142
2	107	72
3	69	60
4	46	51
5	9	50
6	21	55
7	71	60
8	194	85
9	117	153
10	2089	2122

Considering the rough nature of cloudiness observations, the agreement must be considered fairly good, and very probably the smooth results of the theory* are closer to the real facts of the case than the irregular observations. The chief interest of this Note lies, however, in the fact that it shows the capacity of the theory of skew variation already developed to cover novel and unusual types of frequency.

"On the Occlusion of Hydrogen and Oxygen by Palladium."
    By Ludwig Mond, Ph.D., F.R.S., William Ramsay, Ph.D.,
    LL.D., Sc.D., F.R.S., and John Shields, D.Sc., Ph.D. Received December 8,—Read December 16, 1897.

(Abstract.)

During their investigations on the nature of the occlusion of gases by finely divided metals, and in particular on the occlusion of hydrogen and oxygen by platinum black, the authors have had occasion to examine the behaviour of palladium to these gases.

The palladium was employed in three states of aggregation, viz., in the form of (a) black, (b) sponge, and (c) foil. Palladium black, prepared in the same way as platinum black, contains 1·65 per cent. of oxygen, or, taking the density of palladium black as 12·0, 138 volumes of oxygen. It differs from platinum black, however,

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* The diagram on which the percentile curves are roughly drawn also indicates the amount of agreement by a histogram.

inasmuch as the oxygen cannot be removed *in vacuo* at a dull red heat, and consequently had to be determined in the ignited substance by passing hydrogen over it and weighing the water produced. Palladium black dried at 100° contains 0·72 per cent. of water, and hence, on the assumption that the oxygen exists as PdO, we have for the analysis of palladium black—

> Pd .... 86·59 per cent.
> PdO ...... 12·69   „   = 1·65 per cent. O_2
> H_2O .... 0·72   „

On heating in an atmosphere of oxygen, palladium black goes on absorbing oxygen at least up to a red heat, with the formation of a brownish-black substance, which does not again lose its oxygen at a dull red heat *in vacuo*. The amount of oxygen absorbed (nearly 1000 volumes) was about one and a half times as much as corresponds with the formula Pd_2O, and if the ignition had been sufficiently prolonged, the whole of the palladium would probably have been converted into the oxide PdO.

Palladium black, when exposed to hydrogen gas, absorbed over 1100 volumes, but of this only 873 volumes were really occluded, the remainder having formed water with 139 volumes of oxygen originally contained in the black, which is in good agreement with the direct gravimetric estimation.

Of the hydrogen occluded, about 92 per cent. was pumped off slowly at the ordinary temperature, and almost the whole of the remainder at 444°. Increase of pressure of the hydrogen from one atmosphere up to 4·6 atmospheres had no influence on the quantity occluded at the ordinary temperature.

The pure palladium sponge remaining in the experimental tube after the above experiment was over occluded 852 volumes of hydrogen, and about 98 per cent. of this was extracted *in vacuo* at the ordinary temperature.

New palladium foil behaved in a very peculiar fashion. At first it scarcely occluded any hydrogen even after ignition in the gas and subsequently cooling down. It was therefore charged and discharged several times electrolytically with hydrogen, but still it persistently refused to occlude any appreciable quantity when replaced in an atmosphere of hydrogen.

After powerful ignition in the blowpipe flame, when it was probably oxidised and then again reduced at a still higher temperature, it was introduced once more into the experimental tube. It immediately occluded a considerable quantity of hydrogen, and by maintaining the temperature between 100° and 130°, a large additional quantity was slowly absorbed. On cooling down to the ordinary temperature, *hydrogen was again occluded, and it was finally found*

to have taken up 846 volumes, *i.e.*, approximately the same quantity as the black or sponge.

The hydrogen occluded by palladium foil is given off again very slowly at the ordinary temperature *in vacuo*, but rapidly and almost completely at 100°.

The paper contains some attempts to explain the extraordinary behaviour of palladium foil.

The heat evolved on the occlusion of hydrogen by palladium black was measured in an ice calorimeter (temperature of the room 20—24°) in nearly the same way as the corresponding heat of occlusion of hydrogen by platinum black, thereby avoiding errors due to the pre-existence of oxygen in the substance.

Favre's statement that the heat of occlusion remains constant for the different fractions of hydrogen occluded was confirmed, and it was found that $+46.4$ K (4640 g. cal.) were evolved per gram of hydrogen occluded.

The authors consider that this number may be taken as correct within 1 per cent., and compare it with the different values found by Favre and those calculated by Moutier and Dewar.

If the external work done by the atmosphere be eliminated, the heat evolved per gram of hydrogen occluded becomes $+43.7$ K.

The heat evolved per gram of oxygen absorbed was also determined in an indirect manner, and found to be $+11.2$ K (1120 g. cal.).

This number, referred to 16 grams of oxygen, lies intermediate between the values given by Thomsen for the heat of formation of palladious and palladic hydroxides, and may be consistent, considering the accuracy of such measurements, with the formation of either of these hydroxides or with a mixture of both. In any case it is of the same order of magnitude, and taken in conjunction with the behaviour of palladium black when heated in an atmosphere of oxygen, is undoubtedly in harmony with the view that the absorption of oxygen by palladium black (and probably also by platinum black) is a true phenomenon of oxidation.

The authors have also investigated the atomic ratio—palladium : hydrogen for fully charged palladium black, sponge, and foil, and give in tabular form the corresponding ratios deduced from experiments by Graham and Dewar in which wire and block palladium were charged with hydrogen electrolytically. They have arrived at the conclusion that no matter whether the palladium exists as black, sponge, foil, wire, or compact metal, or whether it is charged by direct exposure to hydrogen gas (the proper conditions being observed), or charged electrolytically, the amount of hydrogen occluded in each case is approximately the same, the atomic ratio varying between 1·37 and 1·47.

Hoitsema has shown that Troost and Hautefeuille's deduction that a compound exists having the formula Pd_2H is not warranted. The constancy of the heat of occlusion over the whole range of absorption is also opposed to the view that such a compound is formed.

The composition of fully charged palladium hydrogen corresponds closely with the formula Pd_3H_2 first suggested by Dewar. The principal and almost only evidence, up to the present, in favour of the formation of such a definite chemical compound is to be found in the approximation of the above atomic ratios to the theoretical value 1·5, required by the formula Pd_3H_2. Although Hoitsema's arguments may be equally well directed against the existence of this compound, the authors consider that additional and independent evidence is desirable, and hope to be able to provide it.

It is also shown that the heats of occlusion of hydrogen in platinum and palladium black are not in favour of the view which has sometimes been put forward that the heat of occlusion of a gas represents the heat of condensation or liquefaction of the gas in the capillary pores of the absorbing substance, or the heat of solidification or fusion.

" On the Determination of the Indices of Refraction of various Substances for the Electric Ray. II. Index of Refraction of Glass." By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by LORD RAYLEIGH, F.R.S. Received October 1,—Read November 25, 1897.

In my previous paper, read before the Royal Society on October 20, 1895,[*] I described a method of determining the indices of refraction of various substances for electric radiation, the principle of which depends on the determination of the critical angle at which total reflection takes place. A semi-cylinder of the given substance was taken, and the angle of incidence gradually increased till the rays were totally reflected. The experiment was repeated with two semi-cylinders, separated by a parallel air-space. The advantage of the latter arrangement was that the image cast by the two semi-cylinders remained fixed. The image underwent extinction when the angle of incidence attained the critical value.

The determination of the indices of refraction for long electric waves derives additional interest from Maxwell's theoretical relation between the dielectric constant and the refractive index for infinitely long waves. The relation $K = \mu^2$ has, however, been found to be fulfilled in only a few instances. The value u_x is usually deduced

from Cauchy's formula, which is admittedly faulty when applied to rays below the visible spectrum. It would therefore be of interest to be able to measure *directly* the index for long electric waves, and compare it with the value of K for rapidly alternating electric fields, the periodicity of which is preferably of the same order as that of the electric waves for which the index is determined.

Among the substances in which great divergence is exhibited between the values of K and μ^2, glass may be taken as typical. In the very carefully conducted series of experiments by Hopkinson the value of K (later results) was found to be 6·61 for light flint and 9·81 for extra dense flint glass. He found no variation of K with the time of charge, which varied from 1/4 to 1/20,000 part of a second.[*] Messrs. Romich and Nowak[†] found the value to be 7·5 for alternation of field of about once in a second, while for steady fields they obtained the abnormally high value of 159. Schiller[‡] found K for plate glass to be 6·34, with a frequency of alternation of 25 in a second. With a higher frequency of about $1·2 \times 10^4$, the value obtained was lower, *i.e.*, 5·78. Gordon, with a frequency of $1·2 \times 10^4$, obtained 3·24 as K for common glass.

From the experiments of Schiller it would appear that the value of K for glass diminished with the increase of frequency of alternation of the field.

Rubens and Arons[§] compared the velocities of propagation of electro-magnetic action through air and glass, and obtained the ratio of the velocities or $\mu = 2·33$. The deduced value of K would therefore be 5·43. M. Blondlot[||] found K to be 2·84 when the frequency of vibration was of the order $2·5 \times 10^7$. Professor J. J. Thomson found the specific inductive capacity of glass to be smaller under rapidly changing fields than in steady ones. He deduced the value of K by measuring the lengths of wave emitted by a parallel plate condenser with air and glass as dielectrics. The value for glass was found to be 2·7.[¶]

On the other hand, Lecher[**] found that the dielectric constant rose with the frequency of vibration. Thus for plate glass—

Frequency.	K.
2	4·64
2×10^3	5·09
$3·3 \times 10^6$	6·50

[*] Hopkinson, 'Phil. Trans.,' 1881, Part II.
[†] 'Wien. Ber.,' vol. 70, 1874.
[‡] 'Pogg. Ann.,' p. 152, 1874.
[§] 'Wied. Ann.,' vol. 42, p. 581; vol. 44, p. 206.
[||] 'Compt. Rend,' May 11, 1891, p. 1058.
[¶] 'Roy. Soc. Proc.,' vol. 46, p. 292.
[**] 'Phil. Mag.,' vol. 31, p. 205.

There is thus a serious difference between the two views of the variation of K (and therefore of μ) with the frequency of vibration. In a previous paper,[*] I alluded to the probability of the variation of μ with the frequency of vibration. The value of μ may at first undergo a diminution with the increase of frequency, reach a minimum, and then have the value augmented when the frequency rises above the critical rate. The result obtained by Lecher is, however, too divergent from the others to be explained by such a supposition.

The direct determination of μ for glass for electric oscillations of high frequency, seemed to me of interest, as throwing some light on the controversy; so, on the conclusion of my determination of the index for sulphur, I commenced an investigation for the determination of μ for glass. This was, however, greatly delayed by repeated failures to cast glass here, and by my long absence from India. I have now obtained from England two semi-cylinders of glass, with a radius = 12·5 cm. and height = 8 cm.

The method of experiment followed is the same as that described in my previous paper. The radiator is placed at the principal focus (obtained from a preliminary experiment) of one of the semi-cylinders. The cylinder mounted on the platform of a spectrometer is rotated till the rays are totally reflected. From the critical angle the value of μ is deduced.

I shall here describe some modifications introduced in the apparatus, which have been found to be great improvements. One of the principal difficulties met with was in connexion with the disturbance caused by stray radiation. It is to be remembered that the receiver is extremely sensitive. Comparatively long waves are found to possess very great penetrative power; shielding the receiver then becomes very difficult. Even after the receiver, the galvanometer, and the leading wires had been screened, disturbances were met with which it was difficult to localise. Part of the disturbance may have been due to that set up by the generating coil. A double box made of soft iron and thick copper removed this difficulty. But the greatest immunity from disturbance was secured by using short waves. In this case it was not at all necessary to take very special precautions to shield either the galvanometer or the leading wires, the sensitive layer in the receiver alone being affected by the radiation. I exposed the bare leading wires to the strong action of the radiator by putting them in close proximity to the source of radiation, and yet no response was observed in the galvanometer. This freedom from disturbance is not due to the opposite action on the two wires, for a single wire may be exposed to the radiation without any action on the receiver.

* *Vide* 'Roy. Soc. Proc.,' vol. CO, p. 168.

x 2

With small radiators the intensity of radiation is not very great. This is a positive advantage in many experiments. It sometimes becomes necessary to have greater intensity without the attendant trouble inseparable from too long waves. I have made a new radiator, where the oscillatory discharge takes place between two small circular plates 12 mm. in diameter and an interposed ball of platinum 9·7 mm. in diameter. The sparking takes place at right angles to the circular plates. The intensity of radiation is by this expedient very greatly increased.

In my previous experiments to determine the index of refraction, I used tubes to surround the radiator. This I was obliged to do to protect the receiver as much as possible from external disturbances. But this procedure may be open to the objection that the sides of the tube may send reflected waves. It is preferable to have a divergent beam from a single source form a well-defined image after refraction. Owing to the successful removal of the disturbing causes it is now possible to allow the radiator to be placed in open space, a plate with a rectangular aperture allowing the radiation to fall on the refracting cylinder along a given direction. The size of the plate is 26 × 15 cm., and the aperture is 7 × 6 cm. (see fig. 1). The radiator and the receiver are placed on opposite sides of the plate. Absence of disturbance due to lateral waves was tested by closing the aperture and observing whether the waves still affected the receiver by going round the plate. The plate was found to act as an effective screen.

I have hitherto preferred the null method in my experiments, as it possesses many advantages. The sensitiveness of the receiver can be pushed to the utmost extent, and observations taken when no effect is produced on the receiver. The total reflection method also dispenses with the difficulty of making accurate measurement of the deviation produced. After obtaining the value of the index by the method described above, I was desirous to see whether it was not possible to obtain fairly good results by measuring the angle of refraction corresponding to a given angle of incidence. I shall presently describe the difficulties met with in these experiments, and the manner in which they were to a great extent removed.

The preliminary experiment was carried out with a single semi-cylinder. The angle of incidence was gradually increased by rotating the cylinder, and the refracted beam was followed with the receiver. In this way it was found that the rays ceased to be refracted when the angle of incidence was about 28° 30'. The critical angle is therefore 28° 30' and

$$\mu = 2·08 \quad \dots\dots\dots\dots\dots\dots\dots \quad (1).$$

I next used two semi-cylinders. The plane vertical face of the

semi-cylinder near the radiator, was placed along a diameter of the spectrometer circle. The second semi-cylinder was separated from the first by an air-space 2 cm. in breadth. The plane surfaces of the two semi-cylinders were thus separated by a parallel air-space; the first semi-cylinder rendered the beam parallel, and the second focussed the rays on the receiver placed opposite the radiator. With the radiator used, I found a thickness of 2 cm. of air-space to be more than sufficient for total reflection of the incident ray.[*]

On rotating the cylinders to the right and to the left, two positions for total reflection were obtained. The difference of circle readings for these positions, equal to twice the critical angle, was found to be 58°. The critical angle for glass is therefore 29°.

$$\mu = 2\cdot 04 \quad \dots\dots\dots\dots\dots\dots \quad (2).$$

Having thus obtained the value of the index, I tried to find whether it would be possible to obtain approximately good results by measuring the deviation of the refracted ray. In the first series of experiments, I used for this purpose a semi-cylinder, with the radiator at its principal focus (the cylindrical surface being next to the radiator), so that the emergent rays were parallel. On trying to find the angle of refraction corresponding to a given angle of incidence, I could obtain no definite reading, as the receiver continued to respond, when moved through five or six degrees on either side of the mean position where the response was strongest. It must be remembered that owing to the finite length of the waves, there is no well-defined geometrical limit to either the ray or the shadow. There is, however, a position for maximum effect, and it is possible with some difficulty so to adjust the sensitiveness of the receiver that it shall only respond to the maximum intensity.

Another troublesome source of uncertainty is due to the action of the tube which encloses the receiver. When a slanting ray strikes the inner edge of the tube, it is reflected and thrown on to the delicate receiver. Unfortunately it is difficult to find a substance which is as absorbent for electric radiation as lamp-black is for light. Lamp-black in the case of electric radiation produces copious reflection. I have tried layers of metallic filings, powdered graphite, and other substances, but they all fail to produce complete absorption. The only thing which proved tolerably efficient for this purpose was a piece of thick blotting paper or cloth soaked in an electrolyte. A cardboard tube with an inner layer of soaked blotting paper is impervious to electric radiation, and the internal reflection, though not completely removed, is materially reduced. No reliance can,

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[*] *Vide* the following paper "On the Influence of the Thickness of Air-space on Total Reflection of Electric Radiation."

however, be placed on this expedient, when a very sensitive receiver is used.

After repeated trials with different forms of receiving tubes, I found a form, to be described below, to obviate many of the difficulties. Instead of a continuous receiving tube, I made two doubly inclined shields, and placed them one behind the other, on the radial arm which carries the receiver. The first shield has a tolerably large aperture, the aperture of the second being somewhat smaller. The size of the aperture is determined by the wave-length of radiation used for the experiment. It will be seen from this arrangement, that the rays which are in the direction of the radial arm, can effectively reach the receiver, the slanting rays being successively reflected by the two shields. With this expedient, a great improvement was effected in obtaining a definite reading.

When the deviated rays are convergent, the receiver is simply placed behind the shields, at the focus of the rays. But when the rays are parallel, the use of an objective (placed behind the first shield) gives very satisfactory results. As objectives I used ordinary glass lenses; knowing the index from my experiments, I was able to calculate the focal distance for the electric ray. This is of course very different from the focal distance for the luminous rays. I at first used a lens of 6 cm. electric focal distance, but this did not improve matters sufficiently. I then used one with a longer focus, *i.e.*, 13 cm., and this gave satisfactory results.

The receiver used to be enclosed in a metallic case, 2 cm. in breadth, with an open front for the reception of radiation. The case was used to protect the receiver from stray radiations. But by the new arrangement and improved construction, these disturbances were effectively removed. I therefore discarded the use of the metallic enclosing cell, as it seemed to me that the rays which did not actually fall on the sensitive surface might be reflected from the back of the metallic cell and thrown on to the sensitive layer. The layer of spirals, only 1·5 mm. in breadth, is laid on a groove in ebonite (which is transparent). This linear receiver without any metallic case was placed at the focus of the lens.

I now proceeded to measure the angle of refraction corresponding to a given angle of incidence. In the first series observed, the refraction was from glass to air; the cylindrical surface of the semi-cylinder was turned to the radiator, which was placed at its principal focus. The receiver was mounted on the radial arm with the double shields, and the objective in the manner already described. The reading for refracted rays was taken in the following manner. Having adjusted the semi-cylinder for a given angle of incidence, the receiver was moved round till it responded to the refracted ray. Readings were taken first by placing the receiver at an angle less
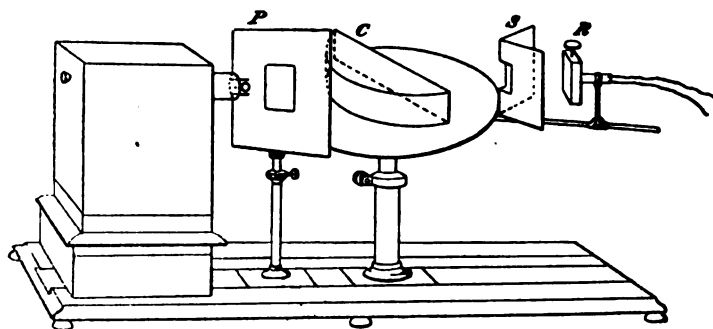
FIG. 1.—The electric refractometer: P, the plate with a diaphragm; C, the semi-cylinder of glass; S, the shield (only one shown in the diagram); R, the receiver.

than the true reading and gradually *increasing* the angle till there was a response. The receiver was then placed at a greater angle, and the angle gradually *reduced* till the receiver again responded. In this way a series of readings for a particular angle of incidence was obtained. These readings were found fairly concordant, the maximum variation from the mean being not so great as 1°. One set of readings being taken on one half of the spectrometer circle, the cylinder was rotated in the opposite direction, and readings taken on the other side.

Angle of Incidence.	Angle of Refraction.			μ.
	Reading to the right.	Reading to the left.	Mean.	
15°	31° 0′ 31 0 31 30 31 30	31° 30′ 30 30 31 30 31 30	31° 15′	2 00
20°	45° 30′ 45 30 44 30 45 30	45° 30′ 46 0 44 0 45 30	45° 15′	2·08
22°	48° 0′ 50 0 49 30 50 0	49° 30′ 50 30 48 30 50 0	49° 30′	2·03

Mean value of $\mu = 2\cdot04$ .... (3).

In the next series of observations, the rays were refracted from air into glass. The electric beam was rendered parallel with the help of a glass lens (f = 4 cm.). The beam was incident on the *plane* face of the semi-cylinder. As the cylinder itself focussed the refracted beam, the objective hitherto used in conjunction with the receiver was dispensed with.

i.	*r.*	Mean value of *r.*	*μ.*
40°	18° 19 18	18° 20'	2·04
50°	22° 23 22° 30'	22° 30'	2·00
65°	25° 30' 26° 27	26° 10'	2·05

Mean value of *μ* = 2·03 ....   (4).

The different values of *μ* obtained are given below :—

From total reflection from a single semi-cylinder, 2·08	....	(1)
„ „ „ two semi-cylinders .. 2·04	....	(2)
From refraction from glass into air .... 2·04	....	(3)
„ „ air into glass .... 2·03	....	(4)

The frequency of vibration was of the order 10^{10}.

The value of the optical index of the glass determined by the total reflection method was found to be

$$\mu_D = 1.53.$$

"On the Influence of the Thickness of Air-space on Total Reflection of Electric Radiation." By JAGADIS CHUNDER BOSE, M.A., D.Sc., Professor of Physical Science, Presidency College, Calcutta. Communicated by LORD RAYLEIGH, F.R.S. Received November 15,—Read November 25, 1897.

In my preliminary experiments on the determination of the index *of refraction of* various substances for electric radiation, I used a

single semi-cylinder of the given substance ; the electric ray was refracted from the denser medium into air, and at the critical angle of incidence it underwent total reflection. The experiment was repeated with two semi-cylinders separated by a parallel air-space. With light waves an extremely thin air-film is effective in producing total reflection. But a question might arise whether waves a hundred thousand times as long would be totally reflected by films of air, and, if so, it would be interesting to find out the minimum thickness of air-space which would be effective in producing this result. This point was raised by Professor Lodge, at the discussion on my paper " On a Complete Apparatus for the Study of the Properties of Electric Waves," read before the Liverpool meeting of the British Association last year. I have for some time past been engaged in an investigation on this subject. The factors which are likely to determine the effective thickness of air-space for total reflection are : (1) the index of refraction of the refracting substance ; (2) the angle of incidence ; (3) the wave-length of the incident electric radiation. In the following investigation, I have studied the influence of the angle of incidence and of the wave-length in modifying the thickness of the effective air-space. The refracting substance used was glass.

I. *Influence of the Angle of Incidence.*

The great experimental difficulty in these investigations lies in the fact, that there is at present no receiver for electric radiation which is very sensitive, and at the same time strictly metrical in its indications. This difficulty is further complicated by the fact that the intensity of the electric radiation cannot be maintained absolutely constant. For these reasons, it is extremely difficult to compare the results obtained from different sets of observations. Attempts have been made in the following experiments to remove, to a certain extent, some of these difficulties.

Two semi-cylinders of glass, with a radius of 12·5 cm., were placed on the spectrometer circle. The plane faces were separated by a parallel air-space. The radiator was placed at the principal focus of one of the semi-cylinders ; the rays emerged into the air-space as a parallel beam, and were focussed by the second semi-cylinder on the receiver placed opposite the radiator. Electric radiation was produced by oscillatory discharge between two small circular plates 1·2 cm. in diameter and an interposed platinum ball 0·97 cm. in diameter.

The two semi-cylinders were separated by an air-space 2 cm. in thickness ; this thickness was found to be more than sufficient for total reflection. The critical angle for glass I found to be 29°. I commenced my experiments with an angle of incidence of 30°

(slightly greater than the critical angle). The receiver, which was placed opposite the radiator, remained unaffected as long as the rays were totally reflected. But on gradually diminishing the thickness of air-space by bringing the second semi-cylinder nearer the first (always maintaining the plane surfaces of the semi-cylinders parallel), a critical thickness was reached when a small portion of the radiation began to be transmitted, the air-space just failing to produce total reflection. The *beginning* of transmission could easily be detected and the critical thickness of air determined with tolerable accuracy. The slight discrepancy in the different determinations was due to the unavoidable variation of the sensitiveness of the receiver. When the thickness of air was reduced to 14 mm., the receiver began occasionally to be affected, though rather feebly. But when the thickness was reduced to 13 mm. there was no uncertainty; a measurable, though small, portion of the radiation was now found to be always transmitted.

I now increased the angle of incidence to 45°, and observed that the minimum thickness, which at 30° just allowed a small portion of radiation to be transmitted, was not sufficiently small to allow transmission at the increased angle of incidence. The thickness had to be reduced to something between 10·3 mm. and 9·9 mm. for the beginning of transmission.

With an angle of incidence of 60°, the minimum thickness for total reflection was found to be between 7·6 mm. and 7·2 mm.

Angle of incidence.	Minimum thickness of air for total reflection.
30°	Between 14 and 13 mm.
45	„ 10·3 and 9·9 mm.
60	„ 7·6 and 7·2 mm.

The minimum effective thickness is thus seen to undergo a diminution with the increase of the angle of incidence.

II. *The influence of the Wave-length.*

In the following experiments I kept the angle of incidence constant, and varied the wave-length. I used three different radiators, A, B, and C; of these A emitted the longest, and C the shortest waves.

The following method of experimenting was adopted as offering some special advantages. If a cube of glass be interposed between *the radiator* and the receiver placed opposite to each other, the

radiation striking one face perpendicularly would be transmitted across the opposite face without deviation and cause a response in the receiver. If the cube be now cut across a diagonal, two right-angled isosceles prisms will be obtained. If these two prisms were now separated slightly, keeping the two hypotenuses parallel, the incident radiation would be divided into two portions, of which one portion is transmitted, while the other portion is reflected by the air film in a direction (see fig. 1) at right angles to that of the
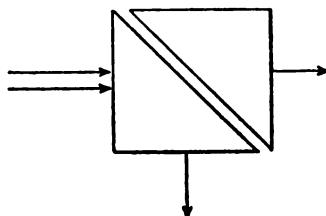
FIG. 1.—Section of the two prisms.

incident ray, the angle of incidence at the air-space being always 45°. The transmitted and the reflected portions would be complementary to each other. When the receiver is placed opposite to the radiator, in the A position, the action on the receiver will be due to the transmitted portion; but when the receiver is placed at 90°, or in the B position, the action on the receiver will be due to the reflected portion. The advantage of this method is that the two observations for transmission and reflection can be successively taken in a very short time, during which the sensitiveness of the receiver is not likely to undergo any great change. In practice three readings are taken in succession, the first and the third being taken, say, for transmission and the second for reflection.

I shall now give a general account of the results of the experiments. When the prisms are separated by a thickness of air-space greater than the minimum thickness for total reflection, the rays are wholly reflected, there being no response of the receiver in position A, but strong action in position B. As the thickness is gradually decreased below the critical thickness, the rays begin to be transmitted. The transmitted portion goes on increasing with the diminution of the thickness of air-space, there being a corresponding diminution of the reflected component of the radiation. When the thickness of the air-space is reduced to about 0·3 mm., no reflected portion can be detected even when the receiver is made extremely sensitive. The reflected component is thus practically reduced to zero, the radiation being now entirely transmitted; the two prisms, in spite of the breach due to the air-space, are electro-optically con-
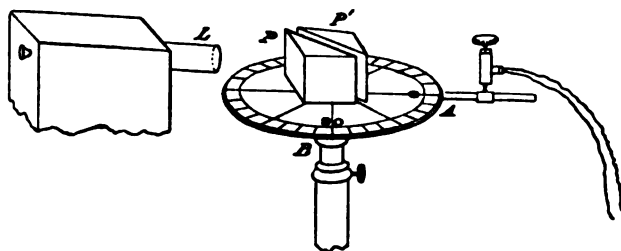
FIG. 2.—L is the lens to render the incident beam parallel; P, P', are the right-angled isosceles prisms; A and B are the two positions of the receiver. The receiver-tube is not shown in the diagram.

tinuous. This is the case only when the two prisms are made of the same substance. If the second prism be made of sulphur, or of any other substance which has either a lower or a higher refractive index, there is always found a reflected portion even when the two prisms are in contact.

Another interesting observation can be made by separating the prisms for total reflection. There would now be no transmitted portion. But if a thin piece of cardboard or any other refracting substance be now interposed in the air-space, a portion of the radiation will be found to be transmitted, and it will be found necessary to separate the prisms further to reduce the transmitted portion to zero.

Having given a general account of the experiments, I shall now describe the method of procedure. The radiator tube was provided with an ordinary lens whose focal distance for electric radiation is about 4 cm. The beam thus rendered approximately parallel fell perpendicularly on the face of the glass prism. The two prisms were made by cutting a cube of glass—an ordinary paper weight— across a diagonal. The size of the cube was 4·5 cm. on each side.*
One prism was fixed on the spectrometer circle; the other could be moved so as to vary the thickness of the interposed air-space between the two sections very gradually. The separation was simply effected by means of ordinary cards. The cards used were of uniform thickness, each card being 0·45 mm. in thickness. A certain number of cards were taken and placed between the prisms with their surfaces in contact with the hypotenuses. The cards were then carefully withdrawn, leaving the prisms separated by a thickness of air equal to the thickness of the given number of cards. It would, of course, be an improvement to have a micrometer screw by which the thickness may be gradually increased.

* Larger prisms would have been preferred, had they been available. The *prisms* after cutting were found to be approximately isosceles, the angles being *90°, 46°, and 44°.*

Observations were now taken to determine the minimum thickness of air for total reflection for different wave-lengths, the angle of incidence being in all cases kept at 45°. Three radiators, R_1, R_2, R_3, were used. I have not yet made determinations of the lengths of wave emitted by these radiators, but it will be seen from the dimensions of the radiators that the waves emitted by R_1 are the longest and those emitted by R_3 the shortest. The oscillatory discharge in R_1 took place between two circular plates 1·2 cm. in diameter and an interposed ball of platinum 0·97 cm. in diameter. The radiators were enclosed in a tube 3·8 cm. in diameter.

In the radiator R_2, the discharge took place between two beads of platinum and an interposed sphere the same as in R_1. The distance between the sparking surfaces was 1·01 cm.

In the radiator R_3, sparking took place between two beads and an interposed sphere 0·61 cm. in diameter. The distance between the sparking surfaces was 0·76 cm.

One prism was fixed on the spectrometer circle, and the other was at first placed somewhat apart from it; the distance was now gradually reduced till the air-space just ceased to reflect totally, when a small portion of radiation began to be transmitted. The beginning of transmission was detected by the receiver, which was placed in the A position. The detection of the beginning of transmission is, as has been said before, somewhat dependent on the sensitiveness of the receiver.

Radiator.	Distance between spark-ing surfaces in mm.	Minimum thickness for total reflection.	
R_1	—	Between 10·3 and 9·9 mm........	(a)
R_2	10·1	„ 7·6 and 7·2 mm........	(b)
R_3	7·6	„ 5·9 and 5·4 mm........	(c)

From the above results it is seen that the effective thickness of the totally reflecting air-space increases with the wave-length. If the wave-lengths are proportional to the distance between the sparking surfaces which give rise to the oscillatory discharge, the wave-lengths in (b) and (c) are in the ratio of 101 : 76. This is not very different from the ratio of the corresponding minimum thicknesses of the totally reflecting air-space.

III. *On the Relation between the Reflected and the Transmitted Components of Radiation when the Thickness of Air-space undergoes Variation.*

In the general account of the experiments, I have said that as the thickness of air-space is gradually reduced the intensity of the

transmitted portion of radiation is increased, while there is a corresponding diminution of the intensity of the reflected portion. This I have been able to verify qualitatively from numerous observations. But in making quantitative measurements many serious difficulties are encountered, owing to the difficulty of maintaining the intensity of radiation, as well as the sensitiveness of the receiver, absolutely constant.

As regards the first, the intensity of the emitted radiation depends on the efficiency of the secondary spark, and the nature of the sparking surface. Keeping the primary current that flows through the Ruhmkorff coil constant, the efficacy of the secondary spark is very much affected by the manner in which the contact is broken in the primary circuit. If a vibrating interrupter is used, the break is apt to become irregular; the torrent of the secondary sparks also spoils the sparking surface of the radiator. For merely qualitative experiments the use of a vibrating interrupter is not so very prejudicial, as along with the ineffective discharges there are present some which are oscillatory. But where successive discharges are to give rise to radiation of equal intensity, it becomes necessary to avoid all sources of uncertainty. For these reasons I prefer a single break for the production of a flash of radiation. With some practice it is possible to produce a number of breaks, each of which is effective. If the surface at the break is kept clean, and the break is properly effected, successive flashes of radiation up to a certain number are about equally intense. When the sparking has been taking place for too long a time, the surface no doubt undergoes a deterioration. But twenty or thirty successive sparks are equally efficacious when sparking takes place between platinum surfaces. The use of a single flash of radiation is preferable on another account. The receiver at each adjustment responds to the very first flash, but becomes less sensitive to the subsequent flashes. The conditions of the different experiments are maintained similar, when the action on the receiver is due to a single flash of radiation, instead of the accumulated effect of an unknown number of flashes.

I give below the deflections of the galvanometer produced by four successive flashes of radiation.

(1) .... 115 divisions.
(2) 122   ,,
(3) 113   ,,
(4) 108   ,,

When very careful adjustments are made, the successive deflections are approximately equal. There are, however, occasional failures, owing either to the fault of the break, or loss of sensitiveness of the receiver.

More serious is the difficulty in connection with the receiver. With the improvements adopted there is no difficulty, under any circumstances, to make the receiver very highly sensitive; but it is extremely difficult to maintain the sensitiveness absolutely uniform. I have in my previous papers explained how the sensitiveness of the receiver depended on the pressure to which the spirals were subjected, and on the E.M.F. acting on the circuit; and how the loss of sensitiveness due to fatigue was counteracted by slightly increasing the E.M.F. For each receiver there is a certain pressure, and a corresponding E.M.F., at which for a given radiation the receiver is sensitive. Having obtained these conditions, the sensitiveness can be increased or decreased to almost any extent by a slight variation of either the pressure or the E.M.F. An increase of pressure produced by the advance of the micrometer press screw through a fraction of a millimetre would sometimes double the sensitiveness; similarly an increase of E.M.F. of even $\frac{1}{100}$ volt increases the sensitiveness to a considerable extent.

The nature of the difficulties in maintaining the sensitiveness of the receiver uniform will be understood from what has been said above. These difficulties are indeed great, and appear at first to be insuperable. But by very careful and tedious adjustments I was able on several occasions to obtain fairly satisfactory results, and was in hopes of ultimately obtaining symmetrical values from the galvanometer deflections. The setting-in of the rainy weather has unfortunately introduced other conditions unfavourable to the maintenance of uniformity of the sensitiveness of the receiver. Owing to the excessive damp and heat the spirals get rusty in a short time, and variation in the sensibility is produced by the altered condition of the surface of the sensitive layer. The results of certain experiments I have carried out lead me to hope that this difficulty will, to a certain extent, be removed by covering the sensitive surface with a less oxidisable coating.

The deflections produced in the galvanometer can only be taken approximately proportional to the intensity of the absorbed radiation. It would be better to observe the diminution of the resistance produced by the incident radiation. This may be done with the help of a differential galvanometer and a balancing resistance.

G is a high resistance differential galvanometer, with two sets of electrodes, A, B; C, D; one pair of electrodes is in series with the receiver, and the other with a resistance box. When the receiver is adjusted to respond to the electric radiation, a weak current flows through it. The same E.M.F. acts on both the circuits. The compensating current, produced by a proper adjustment of the resistance of the box, brings the spot of light back to zero. The resistance of the box is equal or proportional to the resistance of the receiver.

When radiation is absorbed by the receiver the resistance is decreased and this diminution of the resistance is found from the new balancing resistance.
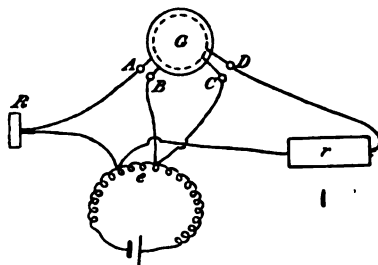
FIG. 3.—G, the differential galvanometer; R, the receiver;
r, the resistance box.

All observations agreed in showing that as the thickness of air-space was gradually decreased, the transmitted component was increased, with a corresponding decrease of the reflected portion. I give below two sets of observations, in which the receiver acted better than usual. The results are to be taken more as qualitative, as no reliance can be placed on the sensibility of the receiver being absolutely uniform.

Radiator R_2; distance between the sparking surfaces = 10·1 mm.

Thickness of air-space in terms of number of cards.	Thickness in mm.	Galvanometer deflection due to the reflected portion.	Galvanometer deflection due to the transmitted portion.
1	0·45	0 or very slight.	Against the stop.
2	0·90	Slight	,, ,,
4	1·8	80	160
8	3·6	145	150
10	4·5	150	120
12	5·4	160	100
16	7·2	Against the stop	30
18	8·1	,, ,,	0

It is seen from the above, that as the thickness of the air-space was gradually increased, the reflected component increased, while the transmitted portion decreased. The minimum thickness for total reflection was found to be about 8 mm. When the thickness of air-space was reduced to about half this thickness (slightly less than half) the reflected and the transmitted portions seemed to be about equal.

With the radiator R_1 the minimum thickness for total reflection was found to be about equal to the thickness of 22 cards (9·9 mm.). When the thickness of air-space was reduced to the thickness of 10 cards (4·5 mm.) the reflected and the transmitted portions seemed to be about equal. As two experiments immediately following each other are more likely to be comparable, the experiments were so arranged that the observation of deflection for *transmission* with a certain thickness of air followed the observation for *reflection* with a different thickness, the corresponding deflections being about equal. As stated above, the reflected and the transmitted portions were approximately equal when the thickness of air was equal to the thickness of 10 cards. Keeping 10 as the mean, pairs of readings were taken with different thicknesses. For example, the reflection reading with a thickness of air equal to the thickness of 4 cards was followed by taking a reading for transmission, with a thickness of air equal to the thickness of 16 cards; the deflections produced in the two cases were about equal, *i.e.*, sixty-six divisions of the scale.

I append below a table showing the corresponding thicknesses of air (in terms of number of cards) which gave approximately equal deflections, the deflection in one case being due to the reflected component, and in the other case to the transmitted component. The receiver was made moderately sensitive, so that the deflections lay within the scale.

Thickness of air for reflection.	Thickness of air for transmission.	Deflection produced.
4	16	66
6	14	70
8	12	90
10	10	120

When the thickness of air was reduced to 0·45 mm., a deflection of

two divisions was obtained for the reflection reading. From this an approximate idea of the intensity of the reflected component may be obtained. Half the total radiation gave a deflection of 120 divisions. The intensity of the reflected component, with a thickness of 0·45 mm., is therefore 1/120th part of the total amount of incident radiation, on the assumption, which is only approximate, that the galvanometer deflections were symmetrical. When the thickness was reduced to 0·3 mm., no reflected component could be detected, though the receiver was made extremely sensitive.

"An Examination into the Registered Speeds of American Trotting Horses, with Remarks on their value as Hereditary Data." By FRANCIS GALTON, D.C.L., F.R.S. Received November 29,—Read December 16, 1897.

It is strange that the huge sums spent on the breeding of pedigree stock, whether of horses, cattle, or other animals, should not give rise to systematic publications of authentic records in a form suitable for scientific inquiry into the laws of heredity. An almost solitary exception to the disregard, shown by breeders and owners, of exact measurements for publication in stud books, exists in the United States with respect to the measured speed of "trotters" and "pacers" under defined conditions. The performance of ·1 mile by a trotter, harnessed to a two-wheeled vehicle, carrying a weight of not less than 150 lbs. inclusive of the driver, in 2 minutes 30 seconds qualifies him for entry in the Trotting Register, giving him, as it were, a pass-degree into a class of horses whose several utmost speeds or "records" are there published. To avoid prolixity I will not speak particularly of pacers (pace = amble), since what will be said of the trotters applies in general principle to them also.

The great importance attached to high speed, and the watchfulness of competitors, have resulted in evolving a method of timing trotters which is generally accepted as authoritative. The length of the track is scrupulously measured, and numerous other conditions are attended to, that shall ensure the record being correct, with an attempted exactitude to the nearest quarter of a second. A race against time, even if exact to the nearest quarter of a second, is by no means so close a measure of the speed of a horse relatively to his competitors, as the differential method of ordinary races. The speed of 1 mile in 2' 30", or of 1760 yards in 150 seconds, is equivalent to about 12 yards in 1 second. Now, the length of a horse when extended at full trot is half as long again as his height at the withers—as I gather from the instantaneous photographs of Muybridge—and consequently is hardly ever as much as

3 yards. Therefore at a 2' 30" speed a horse travels through his whole length in a quarter of a second. In an ordinary English race a winner by half a length gains a notable victory, while a neck or even a head in advance is sufficient to establish his priority. Therefore the record of the speed of a horse to the nearest quarter of a second is by no means an absurd refinement. It is, of course, very difficult under the exciting circumstances of a race to measure time with such precision as that. I tested the value of these entries as follows :—If quarter seconds were noted with exactness the entries of 0, $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ would be approximately equal in number ; they would also be equal if they were set down at random without bias, but if there be a bias towards favourite numbers its effects would be apparent. I extracted a few hundred entries, and found the relative frequency of the 0, $\frac{1}{4}$, $\frac{1}{2}$, and $\frac{3}{4}$ to be almost exactly as 1, 3, 2, and 1. Consequently the $\frac{1}{4}$ is on the average three times as great a favourite as either the 0 or the $\frac{3}{4}$, and the $\frac{1}{2}$ is twice as much a favourite as they are. It is evident that the $\frac{1}{4}$ seconds are not strictly trustworthy, but it may well be urged that their entry is preferable to their total disregard.

I was informed that a trifling laxity was tolerated when a horse had just but only just failed to qualify, an allowance of $\frac{1}{4}$ of a second in his favour being commonly made. So that a speed of 2' 30$\frac{1}{4}$" would usually be reckoned as 2' 30". I shall return to this point further on.

The system of timing and of registering records began more than fifty years ago, and was developed and improved by degrees. In 1892 a considerable change was made in the conditions by the introduction of bicycle wheels with pneumatic tyres, which produced a gain of speed, the amount of which is much discussed, but which a prevalent opinion rates at 5 seconds in the mile. Thenceforward the records are comparable on nearly equal terms. All trotting performances up to the 2' 30" standard are registered in the large and closely printed volumes of 'Wallace's Year Book,' published under the authority of the American Trotting Association. Vols. 8—12 refer to the years 1892-6, and it is from the entries in these that the following remarks are based.

The object of my inquiry was to test the suitability of these trotting (and pacing) records for investigations into the laws of heredity. Their trustworthiness was of course one point to be ascertained, another was to obtain a just notion of the proper principle on which marks for speed should be awarded, as, for instance, in the following example :—Suppose a particular ancestor, whom we will call A, of a certain horse has a record of 2' 30", and that another ancestor in the same degree, whom we will call B, has a record of 2' 10", how are their joint influences to be estimated? Will it be the

z 2

same on the average as that of two horses each having the speed of
2′ 20″, or will it be something altogether different? In short, is the
arithmetical the most appropriate mean or not? It would be a
strong presumption in the affirmative, if the relative frequency of
the various speeds should correspond approximately with those
determined by the normal law of frequency, because if they do so
they would fall into line with numerous anthropometric and other
measures which have been often discussed, and which, when treated
by methods in which the arithmetic mean was employed, have
yielded results that accord with observed facts. Whether the
speeds do or do not occur with the normal frequency had therefore
to be ascertained. So my inquiry had two objects: first, did the
run of the observations suggest a tolerably smooth curve? Secondly,
was that curve a tolerable approach to the curve of normal fre-
quency?

The investigation was troublesome and tedious. It was necessary
to pick out from a large collection the names of those stallions,
geldings, and mares (all three being equally efficient trotters), whose
records had been made in the year under consideration, and who also
had arrived at maturity, that is, who were not less than five years
old, and therefore had had time to show their full powers. Had
younger horses been included, the frequency of the slower records
would have been much increased. Assisted by a friend, the
appropriate entries were underlined in the printed volumes, then
one of us read them out, and the other ticked them down in the
appropriate column of a page ruled for the purpose. Finally the
marks in each column were counted. In this way 5705 extracts
were made from the entries for the years 1892–96; they were not
subsequently verified, so some few omissions are probable. Anyhow
they form a fair and large sample, and are quite sufficient for the
present purpose.

The discussion of this material resulted in rather bulky tables,
which it is needless to reproduce here, because their contents are
given in an adequate and much simpler manner by the accompanying
diagrams. The successive columns in the table are represented in
the diagrams by imaginary columns that stand on corresponding
bases. They run as follows :—The first column, counting from the left,
contains the percentage value of all observations recorded as 2′ 29·0″,
29¼″, 29½″, or 29¾″; that is of all under 30 down to 29 inclusive
(the minutes being here omitted for brevity). The second column
referred to 28·0″, 28¼″, 28½″, and 28¾″, and so on with the rest. Con-
sequently the dot in the diagram which indicates the percentage
number of observations, according to the side scale, stands in the
middle of its own imaginary column. For example, that of the
2′ 28″ set stands vertically above the point that lies half way

between 28 and 29 on the scale along the base. The dots are connected by thin lines to show the trace or curve of the observations. The smooth curves are those of normal frequency, calculated from the values of the mean (M) and of the probable error (P.E.), which are given in the diagrams.
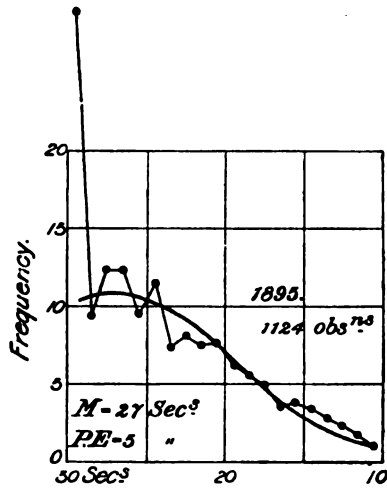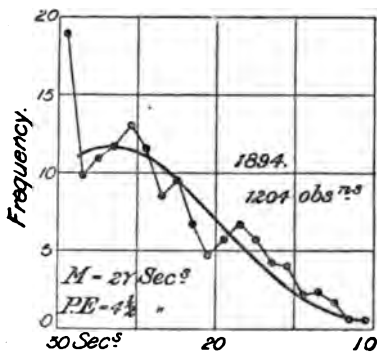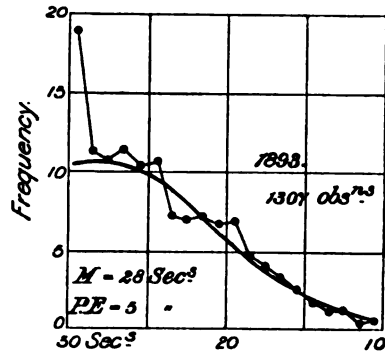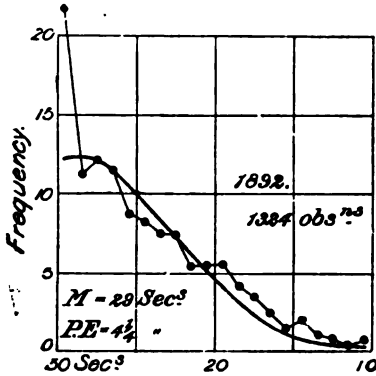
Leaving aside for the moment the strange pinnacle that rises on the extreme left of every diagram, we see that the traces of the observations run very roughly, but not intolerably so. In each diagram they seem to be disposed about a fundamentally smooth curve. Considering the smallness of the interval, namely, only 1 second, that separates the observations assigned to each pair of successive columns, together with the experience derived from other kinds of statistical curves, it seems to me that the run of the obser-

vations is good enough to certify their general trustworthiness.
As regards the pinnacle it is a different matter, and is one which
when beginning work, as I did, on the 1892 entries only, was very
perplexing. However, by persevering with the other years it became
increasingly plain that the pinnacle was a false maximum; in 1896
it was certain that the true maximum lay well within the portion of
the curve included in the diagram. The explanation of the pinnacle
then became obvious; it was that the tolerance granted to those
horses who failed by only a little to qualify themselves, was extended
considerably beyond the quarter second for which I was prepared.*
The cases of 2′ 30·0″ were few; they do not appear in the diagram,
but their addition would be quite insufficient to remove the difficulty.
If the pinnacle were distributed among *two* adjacent columns outside
and to the left of the diagram it would smooth away the incongruity,
so I suspect that cases of "under 2′ 32″ and down to 2′ 30″" are
habitually rated at a trifle less than 2′ 30″. Consequently I had no
hesitation in wholly disregarding the entries that helped to make the
pinnacle, namely, the whole of those contained in the first column to
the left in every one of the diagrams. The course thereupon became
clear and straightforward. When fixing upon the mean for each year,
I was somewhat biassed by the entries in the adjacent years; simi-
larly as to the probable error. Now that the curves are drawn I see
that somewhat better fits might have been made, but they are close
enough to show the existence of a fair amount of correspondence
between the observed values and those calculated according to the
law of normal frequency. It is near enough to remove hesitation in
working with the arithmetic mean.

* [*Jan.* 20.—I have since learnt that the conditions of timing are too rigorous to
justify this inference; also that the very numerous efforts simply to secure a
standard record, and thenceforward to cease training, may be a chief cause of the
pinnacle.]

I now come to the fundamental purpose of this memoir, which is to point out the existence in the registers of the American Trotting Association, of a store of material most valuable to inquirers into the laws of heredity, which accumulates and increases in value year by year. Unfortunately it lies buried to a hopeless depth, partly because the published part of the registers refers only to standard trotters. It appears to be buried simply through the omission of having its importance insisted on. The published volumes of the 'Trotting Register' contain numerous elaborate tables, but lacks one that should include the names and pedigrees of those horses concerning whose antecedents enough is known to make their pedigrees serviceable to investigators.

It is hardly worth while to discuss hereditary influence on speed, in the case of any horses, unless the records of at least their sires and of their dams, and those of each of their four grandparents, as well as their own record, are all known. Even in this case (according, at least, to my own theory) one quarter of the hereditary influences are unknown and have to be inferred. It is practically impossible to make an adequate collection of the names of horses who fulfil the above conditions out of the entries in the 'Trotting Register,' each search requiring many cross references and occupying a long time, while the number of futile searches before attaining a success is great. On the other hand, the breeders and possessors of these notably bred horses must be familiar with the required facts, and would assuredly be delighted to have them known. There need, therefore, be little difficulty in obtaining materials for the much desired table. In the meantime I am sending circulars to the chief breeders in America in hopes of making a start.

The great need for genealogical data of an exact numerical kind, by those who prosecute inquiries into the laws of heredity, is the justification that I offer for submitting these remarks to the Royal Society.

January 20, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

The Right Hon. Sir Nathaniel Lindley, Master of the Rolls, one of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. " The Relations between Marine Animal and Vegetable Life."
    By H. M. VERNON, M.A., M.B. Communicated by Professor
    J. BURDON SANDERSON, F.R.S.

II. " The Homogeneity of Helium."   By WILLIAM RAMSAY, Ph.D.,
    LL.D., Sc.D., F.R.S., and MORRIS W. TRAVERS, B.Sc.

III. " Fergusonite, an Endothermic Mineral."   By WILLIAM RAMSAY,
    Ph.D., LL.D., Sc.D., F.R.S., and MORRIS W. TRAVERS, B.Sc.

IV. " On the Modification of the Spectra of Iron and other Sub-
    stances, radiating in a strong Magnetic Field."   By THOMAS
    PRESTON, M.A.   Communicated by Professor G. F. FITZ-
    GERALD, F.R.S.

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" The Homogeneity of Helium."   By WILLIAM RAMSAY, Ph.D.,
    LL.D., Sc.D., F.R.S., and MORRIS W. TRAVERS, B.Sc.   Re-
    ceived December 2, 1897,—Read January 29, 1898.

About a year ago, a paper by Dr. Norman Collie and one of the authors (W. R.) was published, bearing the title " The Homogeneity of Helium and of Argon."   In that paper* various reasons were adduced to show why an attempt to determine whether or no argon and helium are homogeneous was worth making.   The results of the experiments at that time indicated that while it did not appear possible to separate argon into two portions of different densities, the case was different with helium.   Samples were obtained after

* ' Roy. Soc. Proc.,' vol. 60, p. 206.

repeated diffusion which possessed respectively diffusion rates corresponding to the densities 2·133 and 1·874. It was there pointed out that these densities are not correct (although their ratio is probably not wrong), owing to the curious fact that the rate of diffusion of helium is too rapid for its density, *i.e.*, it does not follow Graham's law of the inverse square root of the densities. These samples of gas also differed in refractivity, and the difference was approximately proportional to the difference in density.

Towards the end of the paper, the conjecture was hazarded that it was not beyond the bounds of possibility that the systematic diffusion of what we are accustomed to regard as a homogeneous gas, for example, nitrogen, might conceivably sift light molecules from heavy molecules. It is true that the fineness of the lines of the spectrum would offer an argument in favour of the uniformity of molecular weight; but still it is never advisable to assume any physical theory without submitting it to rigorous proof. And it was thought possible that the fractional diffusion to which helium had been subjected might have had the result of effecting such a separation; a separation, not of chemical species, but of molecular magnitude. The other and more ordinary explanation of the splitting of helium into fractions of different density is that helium must be regarded as a mixture of two gases, one lighter than the other.

Since the publication of the paper mentioned, Dr. A. Hagenbach has confirmed the possibility of separating helium into portions of two densities by diffusion; and the differences in density were practically the same as those observed in the laboratory of University College.*

These experiments were made with somewhat over 200 c.c. of gas; but it was decided to make experiments of a similar kind, on a much larger quantity of helium.

An apparatus was therefore constructed, similar in principle to the one previously employed, but on a much larger scale. The main features are shown in the illustration on p. 208 of the paper previously alluded to; but on account of the large amount of gas diffused, it was not practicable to collect it in tubes. Instead, therefore, of the bent tube EN of the former apparatus, the tube connected with the stopcock E was continued horizontally, and by means of six vertical branches it communicated with six gas reservoirs, each furnished with a two-way stopcock. It was possible with this means to cause gas from any one of the reservoirs to enter the diffusion apparatus A. In order to be able to collect the gas in any desired reservoir, the delivery tube of the Töpler pump F delivered gas into a jar somewhat similar to that shown at J, but provided with a vertical branch, which was bent horizontally some

* 'Wied. Ann.,' vol. 60, p. 124.

distance up, and lay parallel to the previously mentioned horizontal tube. It, too, had six vertical branches, each of which communicated with the other limb of the two-way stopcock of each reservoir. By raising the reservoir of the Töpler pump and expelling gas into the collecting tube J, the gas could be transferred to any one of the reservoirs. The accompanying diagram makes it clear how the apparatus was set up.
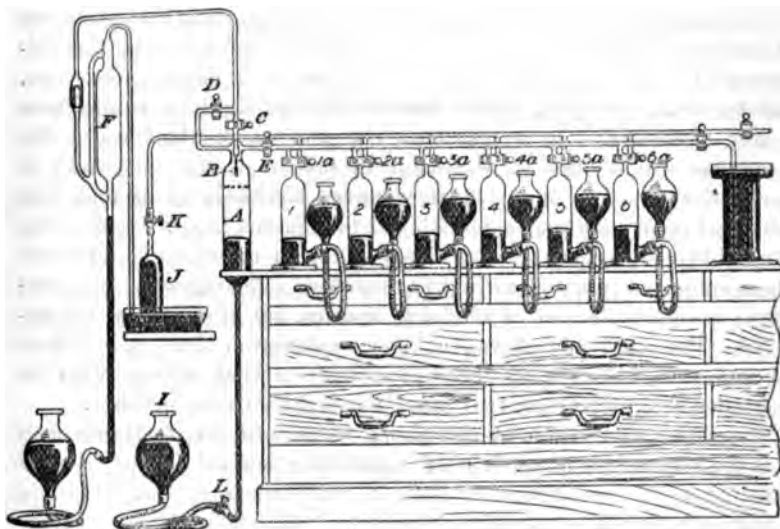
The actual method of conducting a diffusion was as follows:—

Reservoir I was raised until the mercury in the diffusion jar A stood at the level of the dotted line. The clip L was then closed, and the stopcocks C and D opened. The Töpler pump was then worked until all gas was removed from A; the gas, if air (as at the commencement of the whole series of operations), being allowed to escape by moving the collecting jar J, so that it no longer covered the end of the exit tube of the Töpler pump. Stopcocks C and D were next closed, and stopcocks E and 6a opened, so that the gas from 6 entered the diffusion vessel A. By raising the reservoir belonging to 6, all gas was expelled through E into A, clip L being opened meanwhile. Reservoir 6 was now full of mercury, and all gas was in A. Stopcock C was then opened, and the gas in A diffused through the pipe stem B (closed at one end by means of an oxyhydrogen blowpipe) into the pump. This diffusion proceeded until half the gas in A had passed into the pump reservoir F. Stopcock C was then closed, and the Töpler was worked, the diffused gas being delivered into J. Stopcock 6a was then opened, and the reservoir of

6 lowered, so that the gas in J passed into 6. This stopcock was then shut. The contents of 5 were then transferred in a similar manner into A, and one-third of the gas was diffused into the pump. It was collected as before in 6. The diffusion jar A now contained as much gas as had been present in 5. The contents of 4 were next added; half of this was removed by diffusion and transferred to 5. The contents of 3 were added; half was diffused and transferred to 4. The contents of 2 were added; half was diffused and collected in 3. And, lastly, the contents of 1 were added, and the half diffused collected in 2. Stopcock D was then opened, and the mercury in the diffusion jar A allowed to run up to the dotted line; the clip L was closed. All gas was pumped out of A and collected in 1; this constituted one complete round.

As it was not possible to empty the tube issuing from J completely of gas by lowering the reservoir of 1, and as, if not emptied, the heavy gas would have contaminated the light gas from 6 during the next round, the following method was made use of. The gas from 6 was transferred to the empty reservoir A; and then, by lowering the reservoir of 6, mercury rose in the tube issuing from J, and expelled all the heavy gas in the connecting tubes into 6. The clip K was then closed, and by opening the stopcocks 1a and 6a, so that communication took place between jars 1 and 6, the small quantity of gas in 6 was transferred to 1. The apparatus was now ready for a second round.

The Fractional Diffusion of Air.

In order to test the working of the apparatus, a set of diffusions was carried out with air. After four rounds, comprising twenty-four diffusions, the light portion contained 17·37 per cent. of oxygen and the heavy portion 22·03. A fairly rapid separation was thus being effected, considering the closeness of the densities of nitrogen and oxygen.

The Fractional Diffusion of Nitrogen.

A similar set of experiments was carried out with nitrogen, prepared by the action of solutions of ammonium chloride on sodium nitrite, in presence of copper sulphate. The gas was dried and passed over red-hot iron prepared by reduction of ferric oxide in order to remove any oxygen or to decompose any oxides of nitrogen which might be present. After thirty rounds, involving 180 operations, the "light" portion of the nitrogen, after purification by circulation over copper oxide, had not altered in density. It must therefore be concluded that nitrogen is homogeneous as regards the relative density of its individual molecules.

The Fractional Diffusion of Helium.

The first sample of helium employed was prepared from samarskite and cleveite. After seventeen rounds, involving 102 operations, the diffusion rates of the lighter and heavier portions were measured. The first gave a density, calculated from this rate, of 1·807, and the second of 2·128. The same gas was re-diffused until in all thirty rounds had been carried through, involving 180 operations. The light fraction now showed the density (measured diffusion rate against hydrogen) 1·816, and the heavy fraction 2·124. These gases were then circulated; the diffusion rate of the lighter portion pointed to a density of 1·811; the heavier gas was diffused into three portions, of which the more rapidly diffusing had a "diffusion density" of 1·906, and the less rapidly diffusing of 2·032. The lightest gas of all (diffusion density = 1·811) was weighed, and had a "real" density (O = 16) of 2·021; the mixture of the heavy products gave the real density, 2·153. The refractivity of the heavy portion, measured against helium from cleveite, undiffused, yet purified from all removable gases, which had the density (weighed) 2·076, was 1·078, the refractivity of the undiffused gas being taken as unity.

A fresh quantity of helium was next prepared from cleveite, and the former diffused samples were stored in tube-reservoirs for future use. The new helium was washed with caustic soda, but not otherwise purified. This gas was now put through fifteen rounds, comprising ninety operations, and the light portion in jar 6 was purified by circulation over magnesium and copper oxide. Its refractivity was 0·9752 of that of the uncirculated helium. Its density by weighing was 1·979. Owing to the cracking of the glass apparatus the main bulk of the specimen was lost. It may be here interesting to chronicle that the remaining portion was inhaled through the nose and mouth; it possessed neither smell nor taste.

The contents of No. 5 were therefore purified and weighed; its density was 2·049.

The contents of No. 1 were also purified by circulation, and had a gravimetric density of 2·245. It lost on circulation a considerable amount of nitrogen which was estimated as ammonia by treatment of the magnesium containing nitride with water. As we are certain that there was no entry of air in preparing the gas, the 34 c.c. of nitrogen must have been evolved from the mineral. It may have been occluded on the surface of the powdered mineral; it need not be remarked that before heating the mineral a nearly perfect vacuum was made in the tube, and that there was no leakage during the operations. We have previously found traces of nitrogen in gas prepared from cleveite; but not all specimens give off that gas. Supposing, however, to take the worst view, the nitrogen had been

derived from leakage of air, it would correspond to only 0·3 c.c. of · argon.

The contents of jar No. 2 were also purified and weighed. During the purification hardly a trace of nitrogen was removed. The density was 2·209. We have thus :—

    Jar No. 1 contains gas of density...... 2·245
     ,,   2   ,,   ,, ... 2·209
     ,,   6   ,.   ,, ... 1·979

The light gas which had previously been stored in tubes was now mixed with the light gas from the second set of diffusions, and the mixture was re-diffused fifteen times, involving ninety operations. The density of the lightest portion of this helium was determined by weighing and found to be 1·988. The helium had, therefore, not been made sensibly lighter by re-diffusion. The mean of the two determinations may be taken as the true density of pure helium; it is 1·98. The refractivity of this sample measured against hydrogen and multiplied by the ratio between hydrogen and air, viz., 0·4564, gives 0·1238. This specimen of light helium of density 1·988 was placed in one of the refractivity tubes, and the lightest helium of the former preparation (density = 1·979) in the other. They had the same refractivity (1000 to 1004). The contents of No. 1, obtained from the mixture of light gases had the density 2·030, showing that only a little heavier material had been withdrawn.

The lighter fractions of helium were then sealed up in glass reservoirs and stored. The heavier portions were placed in the diffusion apparatus and submitted to methodical diffusion.

After fifteen rounds (ninety operations) the heaviest fraction had density 2·275, the lightest 2·08. The refractivity of the heaviest gas was next determined and found to be 0·1327. This gas examined in a Plücker's tube showed brilliantly pure helium lines, but along with these the reds and green groups of argon. Calculating from the density of this gas it should contain 1·63 per cent. of argon according to the equation $1·961x + 20y = 2·275$. Calculating from the refractivity the percentage of argon should be 1·05, from the equation $1·245x + 0·9596y = 13·33$. A mixture of 99 per cent. of the purest helium and 1 per cent. of argon was made, and it showed the argon spectrum with about the same or with somewhat less intensity than the heaviest gas. Finally, the heavy gas was diffused to the last dregs, so that only about 0·5 c.c. remained undiffused ; and this small residue, transferred to a Plücker tube, showed the argon spectrum with only a trace of the spectrum of helium. The yellow line and the bright green line were visible, but feeble. This spectrum was compared with that of a mixture of argon with a trace of helium, and nearly the same appearance was to be seen. With

· the jar in parallel and a spark gap interposed the blue spectrum of argon was equally distinct in both tubes; and, more important still, *there was no trace of any unknown line.* It appears, therefore, that helium contains no unknown gas, nor is it possible to separate it by diffusion into any two kinds of gas; all that can be said is that most minerals which evolve helium on heating also evolve argon in small quantity. This accounts for the difference in density observed in different samples of helium; and in one instance, viz., malacone, the amount of argon evolved on heating the mineral, though small, was much in excess of the helium, so far as could be judged by the spectrum.

In the light of the experiments of which an account has here been given, it is necessary to reconsider the deduction drawn by Professors Runge and Paschen from the complex nature of the spectrum of helium as regards its complex nature. Sir Norman Lockyer has already pronounced in favour of the supposition that helium is a mixture, chiefly on the ground that in the spectra of certain stars some, but not all, of the helium lines are observable. It appears to us that this may well be accounted for by the hypothesis that the differences of temperature and pressure in the stars might produce variations in the spectrum of helium. If a jar and spark gap be interposed while observing the visible spectrum of helium, a profound alteration is to be noticed. The yellow line D_3 is to be seen near the electrodes, and is faint in the capillary portion of the tube, and one of the red lines disappears. The change is not as remarkable as in the case of argon, but is quite distinct and characteristic. Then, as before remarked, the green line becomes relatively stronger at low pressures, so that the light evolved in the tube is no longer the usual brilliant yellow, but dull greenish-purple. Is it not likely that the conditions obtaining in the stars may account for the absence of some of the lines ordinarily visible?

If the hypothesis of Runge and Paschen is correct, then the two gases to which they attribute the complex spectrum of helium must have nearly the same density. It has already been shown that by means of the apparatus used for the fractional diffusion of gases it is possible to effect a fair separation of the constituents of air after a few rounds. If the supposed constituents of helium differ in density in as high a proportion as 14 to 16, it is certain that some separation would have been effected. As there has been no such separation, the legitimate inference is that the density of the two supposed constituents does not differ by so great an amount, or that their existence is imaginary. It appears to us that too little is known regarding the nature of the vibrations which cause spectra to make it legitimate to theorise on the subject. It is surely conceivable that an atom may *possess such a structure* as to render it capable of propagating two

different sets of vibrations, each complete in itself, and each resembling the other in general form. Yet it must be acknowledged that our experiments have not disproved the existence of two gases in helium of approximately the same density; in fact it may be contended that helium is a pair of elements like nickel and cobalt.

We are disappointed in the result of this long research, because we had thought it not improbable that an element of density 10 and atomic weight 20 might prove to be the cause of the fact that different samples of helium possess different densities, according to the mineral from which they are extracted, and also of the separation of helium into portions of different densities by diffusion. We still regard it as by no means improbable that further research will lead to the discovery of the " missing " element, and this appears to be a fitting opportunity of stating our reasons for the belief.

The difference between the atomic weights of helium and argon is $40 - 4 = 36$. Now, there are several cases of such a difference. If we compare the groups of which the first members are fluorine, oxygen, nitrogen, carbon, boron, beryllium, and lithium, we obtain the following table :—

	At. wt.			At. wt.	
Fluorine..........	19·0	16·5	Boron	11·0	16·0
Chlorine..........	35·5	19·5	Aluminium	27·0	17·1
Manganese........	55·0		Scandium	44·1	
Oxygen	16·0	16·0	Beryllium	9·1	15·2
Sulphur..........	32·0	20·3	Magnesium	24·3	15·8
Chromium........	52·3		Calcium	40·1	
Nitrogen	14·0	17·0	Lithium..........	7·0	16·0
Phosphorus	31·0	20·4	Sodium	23·0	16·1
Vanadium........	51·4		Potassium	39·1	
Carbon	13·0	16·3	Helium	4·0	16·0
Silicon...........	28·3	19·8	?	20·0	20·0
Titanium	48·1		Argon............	40·0	

The elements helium and argon have been given a provisional place.

The differences between the extreme members of these small groups are given in the short table which follows :—

Manganese—Fluorine	36·0	Chromium—Oxygen	36·3
Vanadium—Nitrogen	37·4	Titanium—Carbon	36·1
Scandium—Boron	33·1	Calcium—Beryllium	31·0
Potassium—Lithium	33·1	Argon—Helium	36·0

The difference between the atomic weights of argon and helium, it

will be seen, is not far removed from those of the other pairs of
elements. It appears, therefore, not improbable that there should
be an element with atomic weight 20, resembling both argon and
helium in its properties. Yet it is not so certain that the middle
element should resemble argon and helium, for in the table given it
is seen that there are several examples of elements with a middle
place which do not resemble those at the extremes. The question is
perhaps best left open.

It will be remembered that the gases evolved from a great many
minerals and mineral waters have been examined, and that in many
cases they have been found to contain helium and argon. In no
instance up to the present has any sample of the gases evolved on
heating in vacuum been found to show unknown spectrum lines.
The amount of argon, as proved by the account which we have just
given of our experiments, is very small, and in the case of the gas
from cleveite investigated by Langlet it is probable that argon was
almost completely absent, for it possessed the density 2. In mala-
cone, on the contrary, argon is present in larger amount than helium,
although neither gas is obtainable from it in large quantity. It
appears to us not beyond the limit of probability that in some as yet
uninvestigated mineral the middle member of the helium group may
be discovered. When it is considered that germanium, an element
which has been recognised only in one of the rarest of minerals,
argyrodite, is the middle element of the trio, silicon, germanium, and
tin, of which the first and last members are common, it is surely not
unreasonable to hope that the middle member of the helium trio may
ultimately be found. The amount of helium in fergusonite, one of
the minerals which yields it in fair quantity, is only 33 parts by
weight in 100,000 of the mineral, and it is not improbable that some
other mineral may contain the missing gas in still more minute pro-
portion. If, however, it is accompanied in its still undiscovered
sources by argon and helium, it will probably be a work of extreme
difficulty to effect its separation from these gases.

Addendum.—Since this paper was written, Professors Runge and
Paschen, in a communication to the British Association in August of
this year, have withdrawn their contention that helium is a mixture,
or, perhaps more correctly stated, they now ascribe to helium the
same complexity as that of oxygen, the spectrum of which may also
be arranged in two series, each consisting of three sets of lines. As
oxygen has not yet proved to be complex, the surmise that helium is
complex therefore falls to the ground.

"Fergusonite, an Endothermic Mineral." By William Ramsay, Ph.D., LL.D., Sc.D., F.R.S., and Morris W. Travers, B.Sc. Received December 15, 1897,—Read January 20, 1898.

The mineral fergusonite, discovered by Hartwall, occurs in felspar and mica deposits, in the same manner as most of the rare Norwegian minerals, such as euxenite, orthite, samarskite, &c. The position in which such minerals are found, embedded in masses of felspar, or encrusted with mica, leaves the question of their origin an open one. Whether they are deposited in the felspar by water, or whether they are contemporaneous with the felspar, is a matter of speculation. Fergusonite is a black lustrous mineral, not unlike obsidian in outward appearance, but of considerably higher density. Seen under the microscope, even with the highest power, there is absolutely no sign of crystalline structure, though in thin slices the substance is translucent, and transmits yellow-brown light. It is, however, macrocrystalline, occurring in quadratic sphenoids. It is quite homogeneous, and displays no sign of cavities. Like similar minerals, it contains helium, which is expelled on the application of heat.

But this mineral presents a peculiarity, which has led us to publish this note. When heated to a temperature not exceeding 500° or 600°, it suddenly becomes incandescent, and evolves much of its helium; while its density decreases.

The analysis of the mineral was kindly undertaken by Miss Emily Aston, to whom we desire to express our indebtedness. The mineral has been previously analysed by Hartwall, its discoverer, and by Weber, and, for the sake of comparison, we quote the earlier analyses[*] :—

Composition of Fergusonite.

	Miss Aston.	Hartwall.	Weber.
Oxides of niobium and tantalum	40·95	47·75	48·84
Oxides of yttrium, erbium, &c.......	31·09	41·91	38·61
Oxides of cerium, &c...............	13·87	4·68	3·05
Uranium dioxide .... 3·36	—	0·95	0·35
Uranium trioxide........... 3·81	—	—	—
———	7·17		
Titanium dioxide	4·56.	—	—
ZrO_2............................	—	3·02	6·93
Silica............................	1·42	—	—
Ferric oxide......................	1·55	—	—
FeO	—	0·31	1·33
Lead oxide	0·16	—	—
SnO_2............................	—	1·00	0·35
Copper oxide	0·12	—	—
	100·89	99·62	99·46

[*] Rammelsberg's 'Mineralchemie,' p. 401.

The oxides of niobium and tantalum were converted into double fluorides of these metals with potassium fluoride; and on examination of the crystals under the microscope, they were seen to be almost entirely of one form. They were easily soluble in water, and, from previous experience with these compounds, we were able to recognise them as potassium niobium oxy-fluoride. There appears to be hardly any tantalo-fluoride present in the possible mixture. The uranium dioxide was estimated by heating the mineral with dilute sulphuric acid in a sealed tube, and titrating the dioxide with potassium permanganate. The trioxide was calculated by difference from the total uranium. The cerium metals were separated, as usual, by means of a saturated solution of potassium sulphate.

It is thus seen that fergusonite is mainly a niobate of yttrium, containing oxides of uranium, but in no great quantity.

The gases evolved by the incandescence of nearly 5 grams (4·852) of the mineral, heated in a vacuous tube, had the following composition:—

	Total gas. c.c.	Per gram of mineral. c.c.	Per cent.
Helium	5·24	1·080	75·50
Hydrogen	0·38	0·078	5·47
Carbon dioxide	1·19	0·245	17·14
Nitrogen..........	0·13	0·027	1·88
	6·94	1·430	99·99

The remaining mineral was mixed with hydrogen potassium sulphate, and heated to redness. More gas was evolved; oxygen, resulting from the decomposition of the sulphuric anhydride, was present in considerable quantity. The sulphur dioxide and the carbonic anhydride were removed by passing the gases through soda-lime, before it entered the pump; hence they do not appear in the analysis.

	Total gas. c.c.	Per gram of mineral. c.c.	Per cent.
Helium	3·48	0·733	60·3
Nitrogen..........	0·42	0·088	7·3
Oxygen	1·87	0·394	32·4
	5·77	1·215	100·0

The mineral taken weighed 4·744 grams.

The density was determined before and after heating. Great care was taken to make sure of the absence of air-bells, by warming the powdered mineral under water in a vacuum, before weighing it.

Density before heating...... 5·619
„ after „ ...... 5·375

It is thus seen that the mineral loses density on incandescence.

The amount of heat lost by this curious mineral in parting with its helium was determined. The plan of operation was to burn in oxygen a known weight of hydrogen, ascertained by measuring it, under a small platinum crucible, in a calorimeter. The rise of temperature was noted. This operation was repeated several times, so as to standardise the calorimeter. Some grams of mineral were then placed in the crucible, and the operation was repeated; the heat evolved by the incandescing mineral added itself to that from the burning hydrogen, and the rise of temperature was greater. Knowing the heat of combustion of hydrogen, a simple calculation gave the heat evolved by the exothermic change in the mineral. The actual data are as follows:—

	I.	II.	III.	IV.
Rise of temperature per gram of hydrogen	14·65°	14·68°	14·47°	14·56°
Additional rise for 6·0595 grams mineral	2·13° = 0·352° per gram			
Additional rise for 4·0830 grams mineral	1·38° = 0·338° „			
Mean rise per gram hydrogen	14·59°			
Mean rise per gram mineral	0·345°			
Heat of combustion of 1 gram hydrogen	34200 calories.			
Heat of decomposition of 1 gram mineral	809 „			

In these experiments, a correction was of course introduced for the change of temperature of the calorimeter during the experiment, due to the temperature of the surrounding air being higher or lower than that of the calorimeter.

The percentage of helium in the mineral, by weight, is 0·0194, evolved on incandescence, and on further heating, 0·0132; the total percentage is 0·0326.

Dr. Shields was so kind as to determine the specific heat of fergusonite. A Bunsen's calorimeter, in thorough working order, was used. The data are:—

Weight of mineral........................... 8·789 grams.
Temperature before introducing into calorimeter.. 17·3° C.
Deflection (1 mm. = 0·001053 K)............... 154·4 mm.
Mean specific heat between 0° and 17·3° .... 0·1069

Various questions are raised by the behaviour of this interesting mineral. Its evolution of heat, accompanying its parting with helium, suggest the idea that it is a true endothermic compound of helium. Had its density, as is the case with alumina, and with other oxides which rise spontaneously in temperature when heated, increased instead of decreasing, the evolution of heat might justly have been ascribed to polymerisation. But an evolution of heat, accompanied by a *fall* in density, leads to the conjecture that the loss of energy is the result of the loss of helium; and that, conversely, the formation of the compound must have been concurrent with a gain of energy. That the helium is actually in combination, and not retained in pores in the mineral, is evinced by there being no pores in which the helium might be imprisoned. Surface-absorption is equally out of the question, for the mineral is compact. The only remaining possibility is that the helium is in chemical combination. And if this is true, then the compound must be an endothermic one.

The question next arises, with what constituent of the mineral is the helium in combination? This question cannot at present be answered. All that can be said is that the amount of helium does not appear to depend on the total percentage of uranium, although minerals containing uranium usually (probably always) contain this element. Even in English pitchblende there was found a trace of helium. And in malacone, a mineral containing no uranium, a trace of helium was found; also in a specimen of meteoric iron. The presence of niobic and tantalic anhydrides, and of the yttrium group of elements, is also favourable to its presence. But the proportion between the weight of the helium and that of the other elements present makes any calculation of the atomic relations between the helium and the other elements out of the question.

There is one other substance at least which decreases in density while it evolves heat; that substance is water, in changing into ice. The effect of compressing ice is to lower its melting point, and at the same time to reduce its heat of fusion. At a sufficiently high pressure there would be a continuous transition from ice to water, no heat change taking place during the transition. Matters would be in a similar condition to those which accompany the change of a liquid into gas at the critical temperature; the smallest alteration of temperature would be enough to bring about the change. In speculating on the origin of such a remarkable compound, is it not allowable to guess that it represents a condition of our earth realised only before solidification had set in? That these minerals, containing the rare elements, represent a portion of the interior of our planet; and that under the enormous pressure obtaining at the centre, combination with helium was an exothermic event; and that such compounds, having by some unexplained accident come to the surface of the

globe, where they are no longer exposed to such pressure, they have, in consequence of the change, become endothermic? The frequency of the helium spectrum in the stars, and its presence in the sun, makes it less improbable that some such explanation may lie not far from the truth.

There are at least two other minerals, gadolinite and æschinite, which exhibit endothermic properties. But these minerals, instead of decreasing in density on ignition, increase. The following table shows the gases evolved when they are heated, their densities before and after heating, and the loss of weight which they suffer:—

	Gases evolved. c.c. per gram.				Density.		Loss of weight.
	H_2.	CO.	CO_2.	He.	Before.	After.	
Gadolinite	0·700	0·011	1·060	none	4·289	4·371	0·82
Æschinite	0·458	none	0·215	0·243	4·685	4·793	1·018

It is to be noticed that only the æschinite contains helium, and that in very small quantity.

The fact that these minerals increase in density, and that only one yields helium, places them in a different class from fergusonite. Moreover, the rise of temperature is not to be compared to that seen with fergusonite, for the glow is barely visible.

January 27, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Sir Herbert Eustace Maxwell, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society.

The following Papers were read:—

I. "Mathematical Contributions to the Theory of Evolution. On the Law of Ancestral Heredity." By KARL PEARSON, M.A., F.R.S., University College, London.

II. "On the Zoological Evidence for the former Connection of Lake Tanganyika with the Sea." By J. E. S. MOORE. Communicated by Professor LANKESTER, F.R.S.

III. "The Kelvin Quadrant Electrometer as a Wattmeter and Volt-meter." By E. WILSON. Communicated by Dr. HOPKINSON, F.R.S.

IV. "The Magnetic Properties of almost Pure Iron." By E. WILSON. Communicated by Dr. HOPKINSON, F.R.S.

February 3, 1898.

SIR JOHN EVANS, K.C.B., Treasurer and Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Sir Nathaniel Lindley, Master of the Rolls, was admitted into the Society.

The following Papers were read :—

I. "On the Intimate Structure of Crystals. Part I. Crystals of the Cubic System with Cubic Cleavage. Haloid Salts of the Alkalis." By W. J. SOLLAS, D.Sc., F.R.S.

II. "On the Intimate Structure of Crystals. Part II. Crystals of the Cubic System with Cubic Cleavage. Haloid Compounds of Silver." By W. J. SOLLAS, D.Sc., F.R.S.

III. "Comparison of Oxygen with the extra Lines in the Spectra of the Helium Stars, β Crucis, &c.; also Summary of the Spectra of Southern Stars to the $3\frac{1}{2}$ Magnitude and their Distribution." By FRANK McCLEAN, F.R.S.

IV. "Researches in Vortex Motion. Part III. On Spiral or Gyro-static Vortex Aggregates." By W. M. HICKS, F.R.S.

V. "The Pharmacology of Aconitine, Diacetyl-Aconitine, Benza-conine, and Aconine, considered in relation to their Chemical Constitution." By JOHN THEODORE CASH, M.D., F.R.S., and WYNDHAM R. DUNSTAN, M.A., F.R.S.

VI. "Note on the Experimental Junction of the Vagus with the Cells of the Superior Cervical Ganglion." By J. N. LANGLEY, D.Sc., F.R.S.

"Note on the Experimental Junction of the Vagus Nerve with the Cells of the Superior Cervical Ganglion." By J. N. LANGLEY, D.Sc., F.R.S., Fellow of Trinity College, Cambridge. Received January 26,—Read February 3, 1898.

Two experiments were made on cats. The central end of the vagus, cut a little below the larynx, was turned forward and joined to the peripheral end of the cervical sympathetic. The object of the experiments was to see whether the vagus nerve fibres are capable of forming connexions with any of the structures with which the spinal nerve fibres of the cervical sympathetic are normally connected. The results seem to me to be conclusive as regards this point.

The time allowed for regeneration was in one case 73 days, and in the other 123 days. At the end of these periods anæsthetics were again given, and the nerves stimulated.

Stimulation of the sympathetic in the lower region of the neck, i.e., of its central end, gave no effect of any kind. Hence the central end of the sympathetic had formed no functional connexions with the peripheral end.

Stimulation of the sympathetic a little below the superior cervical ganglion caused reflex effects of the kind caused by vagus stimulation. These reflexes were obvious in the case in which 123 days had been allowed for regeneration, less clear in that in which seventy-three days only had been allowed. They ceased on section of the vagus close to the ganglion of the trunk. Thus afferent fibres of the vagus had grown outwards amongst, or joined with, the fibres of the peripheral end of the sympathetic.

The stimulation also caused all the effects normally produced by stimulation of the cervical sympathetic, so that, although the central end of the sympathetic had not joined the peripheral end, the peripheral end had acquired more or less completely its normal function.

Stimulation of the vagus a little below the ganglion of the trunk —the nerve being cut centrally of the point stimulated—caused dilation of the pupil, retraction of the nictitating membrane, contraction of the arteries of the ear, erection of the hairs of the face secretion of the sub-maxillary gland, and the other effects normally caused by stimulating the cervical sympathetic.

After injection of nicotine no effect was obtained by stimulating the nerves centrally of the superior cervical ganglion; the usual effects following when the ganglion itself was stimulated.

Hence, efferent fibres of the vagus had either grown along the

peripheral end of the cervical sympathetic, and formed nerve-endings around the cells of the superior cervical ganglion, or they had united directly with the sympathetic fibres. That the former had taken place I infer from the fact that the regenerated nerve contained medullated fibres larger than those proper to the sympathetic.

I conclude from the experiments that there is no essential difference between the efferent "visceral" or "involuntary" nerve fibres, whether they leave the central nervous system by way of the cranial nerves, by way of the sacral nerves, or by way of the spinal nerves to the sympathetic system. All of these fibres I take to be pre-ganglionic fibres. And I think that any pre-ganglionic fibre is capable, in proper conditions, of becoming connected with any nerve cell with which a pre-ganglionic fibre is normally connected; although apparently this connexion does not take place with equal readiness in all cases. On the whole it appears to me that the functions exercised both by pre-ganglionic and by post-ganglionic fibres depend less upon physiological differences than upon the connexions which they have an opportunity of making during the development of the nervous system and of the other tissues of the body.

A fuller account of the observations will be published in the 'Journal of Physiology,' after some further experiments have been made.

"Researches in Vortex Motion. Part III. On Spiral or Gyrostatic Vortex Aggregates." By W. M. HICKS, F.R.S. Received January 12,—Read February 3, 1898.

(Abstract.)

A portion of the communication (Sect. II) extends the theory of the simple spherical vortex discovered by Hill. The chief part (Sects. I and III) refers, however, to a kind of gyrostatic aggregate. The investigation has brought to light an entirely new system of spiral vortices. To give an idea of the species of motion considered, take the case of motion of an infinitely long cylindrical vortex of sectional radius a. The velocity perpendicular to the axis inside the vortex will be of the form $v = f(r)$ where $f(o) = 0$. Outside it will be given by $v = Va/r$ where $V = f(a)$.

We may, however, have a motion in which the fluid moves parallel to the axis inside the cylinder with rest outside. The velocity will be of the form $u = F(r)$ inside, where $F(a) = 0$, and zero outside. Both $f(r)$ and $F(r)$ are arbitrary functions subject only to the conditions $f(0) = 0$ and $F(a) = 0$. Putting aside for the present the

question of the stability of these simple motions or of their resultant. it is clear that if we superpose the two we get another state of steady motion in which we have vortex filaments in the shape of helices lying on concentric cylindric surfaces. The problem to be considered is whether it is possible to conceive a similar superposition of two motions in the case of any vortex aggregate whose motions are symmetric about an axis.

The general conditions for the existence of such systems are determined in Sect. I, and are worked out in more detail for a particular case of spherical aggregate in Sect. III. It is found that the motion in meridian planes is determined from a certain function ψ in the usual manner. The velocity along a parallel of latitude is given by $v = f(\psi)\rho$ where ρ is the distance of the point from the straight or polar axis. The function ψ satisfies an equation of the form (when expressed in polar co-ordinates)

$$\frac{d^2\psi}{dr^2} + \frac{1}{r^2}\frac{d^2\psi}{d\theta^2} - \frac{\cot\theta}{r^2}\frac{d\psi}{d\theta} = \rho^2 F - f\frac{df}{d\psi},$$

where F and f are both functions of ψ. The case F uniform, and $f \propto \psi$ is treated more fully. If $f = \lambda\psi/a$ where a is the radius of the aggregate,

$$\psi = A\left\{J_2\left(\frac{\lambda r}{a}\right) - \frac{r^2}{a^2}J(\lambda)\right\}\sin^2\theta.$$

The most striking and remarkable fact brought out is that as λ increases we get a periodic system of families of aggregates. The members of each family differ from one another in the number of layers and equatorial axes they possess. According to the number of independent axes they are called singlets, doublets, triplets, &c., in contradistinction to more or less fortuitous or arbitrary compounds of the former, which are considered later and called monads, dyads, triads, &c. Of these families two are investigated more in detail than the others, both because they are specially interesting in their properties and because they serve as limiting cases between the different series. In one family (the λ_2 family) all the members remain at rest in the surrounding fluid. In the other (the λ_1 family) a distinguishing feature, common to all the members, is that the stream lines and the vortex lines are coincident.

The parameter λ gives the total angular pitch of the stream lines on the outer current sheet, although in aggregates with more than one equatorial axis these lines are not one continuous line. The first aggregates—with $\lambda < 5\cdot7637$ (the first λ_2 value)—behave abnormally. Beyond these we get successive series, in one set of which the velocity of translation is in the same direction as the polar motion of the central nucleus, in the alternate set the velocity is opposite, and

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the aggregate regredes in the fluid as compared with its central aggregate.

Suppose the attempt made to obtain sets of aggregates with greater and greater angular pitch. It will be found that as the external pitch of the stream lines increases the equatorial axis contracts and the surface velocity diminishes. On the outer layers (ring-shaped) the spiral pitch is chiefly produced on the inner side facing the polar axis until on the boundary itself the stream lines lie along meridians and the twist is altogether on the polar axis. The pitch can be increased up to a certain limit. As this is done the stream lines and the vortex lines fold up towards one another, coincide at a certain pitch, and exchange sides.

When an external angular pitch of about 330° is attained it is impossible to go further if a simple aggregate is desired. If a higher pitch is desired the aggregate splits into two concentric portions—an inner spherical portion and an outer shell. The central nucleus is similar to those just described—it produces a part of the required pitch.

The outer layer has spirals with the same direction of twist which complete the balance of the pitch. In these, however, the motion is in the opposite direction. With increasing pitch this layer becomes thicker and its equatorial axis contracts relatively to the mid point of the shell until another limit is reached ; the stream and vortex lines again fold together, cross, and expand as this second limit is reached. If a larger pitch still is desired there must be a third layer, and so on. The first coincidence of stream and vortex lines takes place for an aggregate whose pitch is 257° 27'. Whenever a maximum pitch is attained the aggregate is at rest in the fluid. This is first attained when the pitch is 330° 14'. Beyond this there are two equatorial axes. For a pitch 442° 37' the stream and vortex lines again coincide, the internal nucleus gives 257° 27' of the pitch, and the outer shell the remainder, and so on.

At the end of the paper a theory of compound aggregates is developed. It is not worked out in detail in the present communication, but the conditions are determined for dyad compounds, whilst a similar theory holds for triad and higher ones. Each element of a poly-ad may consist of singlets, doublets, &c. The equations of condition allow three quantities arbitrary—as for instance ratio of volumes, ratio of primary cyclic constants, and ratio of secondary cyclic constants. The full development of this theory is, however, left for a future communication.

If we take any particular spherical aggregate with given λ and primary cyclic constant μ, the energy is determinate. We may, however, alter the energy. If it be increased the spherical form *begins to open out* into a ring form whose shape and properties have

not yet been investigated. If the energy be increased sufficiently the aperture becomes large compared with the thickness of the core and approximate calculation is applicable. The differential equation for ψ in terms of toroidal co-ordinates is given, but the full development is left for a future occasion.

In the paper itself the problem is treated purely as a question of hydrodynamics, and the results simply as the properties of certain possible fluid motions. It may not, however, be out of place here to offer a few remarks, of a more speculative kind, on the bearing of the results on physical theories.

In the first place, gyrostatic motion of the kind here considered is not confined to aggregates, which are symmetrical about an axis. Although the theory is very complicated, it is easy to see that they must exist. In the address to Section A, at the Ipswich meeting of the British Association, a vortex cell theory of the ether was indicated. The ether consisted of closely packed elements, each element being a vortex aggregate. To fix ideas, the case of elements of the shape of a rectangular box was taken, although this particular shape is not essential. The vortical motion there considered was not gyrostatic, but it is clear that a gyrostatic modification is possible. The primary rotations must be arranged in opposite directions in alternate cells. This is, however, not necessarily the case with the secondary gyrostatic motion. They may either be or not be in the same direction, although conditions of stability might decide this question. If the common direction is not a necessity, it is easy to conceive that certain operations on boundaries immersed in the ether might make them so, and in this way produce the same effect as vortex filaments stretching between them. Such a theory would not necessitate return vortex filaments such as are required in any theory which attempts to explain electrical actions by such filaments. It is very conceivable that they would produce the stresses along and perpendicular to tubes of force which are required in an electric field. If a cell, such as that of the λ_2 aggregates in this paper, were possible, the necessity that the primary rotations should be alternately directed would not exist, at least so far as continuity of motion had to decide.

In the second place, does the new theory throw any light on a vortex atom theory of matter? In this respect two remarks should be made. The first is, that if vortex atoms are realities the exact quantitative theory developed in this paper cannot accord with actual facts, because it is developed with reference to a surrounding irrotational ether, which cannot be the case in nature. Nevertheless, many of the general properties would doubtless be similar, and possibly the same for aggregates of the λ_2 family.

The second remark is, that the results of the paper refer only to

spherical aggregates, that is, all the various elements are compared, not when their energies are in thermal equilibrium, but in the artificial association such that the energy of each particular element is that which is necessary to give it a spherical shape. Nevertheless, it is possible to get general ideas. The most striking one is the fact of the periodic property of the atoms. The J_2 curve, for instance, or the curve in the figure which shows how the translation velocity alters with increasing pitch of spiral, irresistibly suggests curves connected with the physical properties of the elements. The abnormal commencement, the regular ascending and descending series suggest the connection at once, and open a vista of possibilities before unsuspected. For the reasons mentioned above, it would be waste of time to look as yet for any definite information. Before that can be done we must know more about the conditions of stability, and the behaviour of such aggregates when their energy changes. It is hardly fitting perhaps to indulge in wild speculations in these pages. In doing so, however, I hope they will be taken for what they are intended, merely as vague intimations of possibilities.

Let us then take the well known curve showing how the fusibilities of the elements alter periodically with the atomic weights.

In a solid body the atoms or molecules can have very little translatory motion. They will therefore take such forms, or their energy will be such as to make this translation small. Now take a spherical aggregate. If it has a large translation velocity its energy must be diminished to render this less—it will take a more elongated form with a small velocity of translation. In order, therefore, to fuse the substance more energy must be put into it. Its temperature of fusion is higher. In other words, it is natural to suppose that those atoms, which when in the spherical form have a high velocity, will possess high fusing points, and so on. Without criticising this argument too closely, let us make the assumption that it is so, and see what it leads to.

Now look at the figure which gives the relation between the velocity of translation (ordinates) to the spiral pitch (abscissæ). We are at once struck with the fact that we have aggregates with large maximum velocity followed with sets of small maximum velocity (in the opposite direction). This is one of the most remarkable features of the fusibility curve. Suppose that the curves march together: this supposition enables us to locate roughly the regions in which the elements lie, omitting the early ones as abnormal. If this be done we find the metals lie on the lower peaked parts and the non-metals on the small flat portions above the line of abscissæ. The following results follow:—

The metals belong to aggregates having an even number of layers or axes, i.e., the outer rotational motion is opposite to that at the centre.
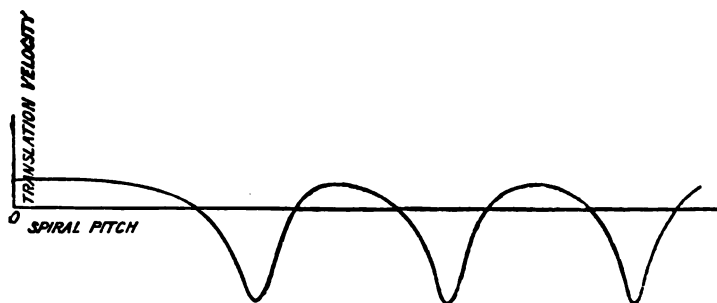
The non-metals belong to aggregates having an odd number of the same, i.e., *the outer rotational motion is in the same direction as that at the centre.*

In the even series of elements (Series 4, 6, 8) the vortex lines lie between the stream lines and the meridians or, as we may express it, *the stream lines lie farthest out.*

In the metals of the odd series the stream lines lie between the vortex lines and meridians, or *the vortex lines are the outermost.*

In the non-metals of the odd series the vortex lines lie between the stream lines and the meridians, or *the stream lines outermost.*

The metals of high fusibility have their stream and vortex lines nearly co-incident. The alkalis have their outer layer thin, the calcium group thicker, and so on.

Having fixed their general position, we may now compare with the curve giving the atomic volumes. When this is done it is found that the atomic volume marches with the moment of angular momentum of the aggregates. In other words—

The moment of momentum due to the gyrostatic effect rises and falls with the volume of the atom.

All that is yet known respecting the stability of vortex rings leads to the conviction that it is not open to us to explain the various densities of matter as we know it by different densities in the material composing the vortex atoms themselves. We must suppose the matter of all atoms to be the same material as the ether itself. The masses must therefore be proportional to the volumes. It follows that atomic volumes, as ordinarily understood, must depend on the spaces occupied in solid bodies by their atoms. Now a ring will clearly take up more space than a sphere of the same volume, and we ought to expect high atomic volumes to go with large aperture rings. Combining this with the last result, it would follow that—

Moment of momentum rises and falls with the equatorial diameter of the ring atom,

which is a highly probable result.

In the present state of the theory, no object is to be gained in pursuing these analogies further. They serve, however, to show directions in which further investigation is to be carried out.

It is clear that if a magnetic field is capable of orienting these aggregates, then a substance composed of them will rotate the plane of polarisation of light.

"The Pharmacology of Aconitine, Diacetylaconitine, Benzaconine and Aconine considered in Relation to their Chemical Constitution." By J. THEODORE CASH, M.D., F.R.S., and WYNDHAM R. DUNSTAN, M.A., F.R.S. Received January 13,—Read February 3, 1898.

(Abstract.)

The investigation which is described in the present paper has been carried out with pure specimens of the alkaloids aconitine, aconine, and benzaconine, the chemistry of which has been fully studied since 1891, by one of us in conjunction with his assistants and pupils, and forms the subject of numerous papers which have been communicated to the Chemical Society, and printed in the 'Journal of the Chemical Society.'[*] As these papers contain a full account of the chemical composition and properties of the various aconite alkaloids, it will not be necessary to do more now than summarise for reference the chief properties of the substances employed in this enquiry.

Aconitine is the poisonous alkaloid contained in *Aconitum napellus*.[†] Commercial specimens of aconitine vary considerably, many of them being mixtures.[‡] Until quite recently the pure alkaloid was not an article of commerce. It is a crystalline base, very sparingly soluble in water, but readily dissolved by alcohol. Its alcoholic solution is dextro-rotatory, whilst solutions of its salts are lævo-rotatory.[§] Even very dilute solutions produce a characteristic tingling and numbness on the tongue and lips. The alkaloid suffers decomposition when heated to its melting point; a molecular proportion of acetic acid is lost, and an alkaloid *pyraconitine* remains.[||] The hydrolysis of the alkaloid occurs in two stages. In the first, which is best effected by heating a salt of aconitine in a closed tube with water,[¶] a molecular proportion of acetic acid is formed, and an

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* 'Chem. Soc. Journ.,' 1891—1897.

† Dunstan and Ince, 'Chem. Soc. Journ.,' 1891, vol. 59, p. 271; Dunstan and Umney, *ibid.*, 1892, vol. 61, p. 385.

‡ Dunstan and Carr, 'Chem. Soc. Journ.,' 1893, vol. 63, p. 491.

§ Dunstan and Ince, *loc. cit.*

|| Dunstan and Carr, *ibid.*, 1894, vol. 65, p. 176.

¶ Dunstan and Carr, *ibid.*, vol. 65, p. 290.

alkaloid produced which is named *benzaconine*, the chief constituent of the *picraconitine* and *napelline* of previous observers.* Further hydrolysis, by alkalis or acids, resolves benzaconine into aconine and a molecular proportion of benzoic acid, and these are the final products of hydrolysis.

A characteristic qualitative reaction of aconitine is the formation of a crystalline purple precipitate of aconitine permanganate when a faintly acidified solution of an aconitine salt is mixed with a solution of potassium permanganate.† Most aconitine salts crystallise well from a solution in water, and in experiments on the physiological action of this alkaloid an aqueous solution of the hydrobromide has been employed.

Neither the composition nor constitution of aconitine can be regarded as settled. In determining the exact formula by which the composition is best expressed, there is the difficulty of deciding between several formulæ which represent the composition of the alkaloid within the limits of experimental error. Alder Wright‡ adopted the formula $C_{33}H_{45}NO_{12}$ as best expressing the composition. Later observers, Jürgens,‡ Lübbe,‡ and ourselves have so far accepted a formula identical with or differing but slightly from that of Wright, as indicating the composition of aconitine and its derivatives. Recently Freund and Beck§ have proposed for aconitine the formula $C_{34}H_{47}NO_{11}$ instead of that employed by us $C_{33}H_{45}NO_{12}$, since they have obtained from the ultimate analysis of the pure alkaloid nearly 2 per cent. more carbon than was found by Alder Wright and his colleagues, by Jürgens, by Lübbe, or by ourselves. The question of composition is, therefore, still unsettled and can probably only be finally decided by the analysis of simpler derivatives of aconitine than have been hitherto dealt with. The constitution of aconitine cannot be considered until more is known of the simpler derivatives and decomposition products. For the purposes of the present discussion it may be regarded as *acetyl-benzaconine*, but nothing is at present known of the constitution of aconine.

Diacetyl-aconitine is an alkaloid obtained from aconitine by acting upon it with acetyl chloride,‖ and differing from it in containing two acetyl groups in the place of two atoms of hydrogen. It is a crystalline base, very sparingly soluble in water, but readily in alco-

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* Dunstan and Harrison, *ibid.*, 1893, vol. 63, p. 443; Dunstan and Carr, *ibid.*, 1893, vol. 63, p. 991; Dunstan and Harrison, *ibid.*, 1894, vol. 65, p. 174.

† Dunstan and Carr, ‘Pharm. Journ.,’ 1896, vol. 56, p. 122.

‡ Alder Wright and Luff, ‘Chem. Soc. Journ.,’ 1877, vol. 31, p. 143; Jürgens, ‘Inaug. Dissert. Dorpat,’ 1885; Lübbe, *ibid.*, 1891.

§ Freund and Beck, ‘Berichte,’ 1894, vol. 27, p. 720.

‖ Dunstan and Carr, ‘Chem. Soc. Journ.,’ 1895, vol. 67, p. 459.

hol. A solution of its hydrobromide in water was used for the determination of its physiological action. This solution, like that of an aconitine salt, produces a persistent tingling and numbness of the tongue and lips.

Benzaconine, the product of the partial hydrolysis of aconitine, occurs with aconitine in *Aconitum napellus,*[*] and is the principal constituent of the substances named napelline and picraconitine by previous observers. It was first named by us *isaconitine,* as its percentage composition was found to agree within the limits of experimental error with that of aconitine.[†] The base is amorphous and separates from a solution in alcohol and ether as a varnish; it dissolves sparingly in water. Solutions of the alkaloid and of its salts are very bitter, but do not produce the tingling of the tongue and lips which is so characteristic of aconitine. Like aconitine, solutions of benzaconine are dextro-rotatory, whilst those of its salts are lævo-rotatory. When hydrolysed benzaconine furnished aconine and benzoic acid. Although the base has not been crystallised, the salts of benzaconine crystallise easily. For the experiments on the physiological action an aqueous solution of the hydrobromide has been employed. Since benzaconine differs from aconitine only in the absence of an acetyl group, attempts have been made to re-form aconitine from benzaconine by replacing this group. These attempts have, however, failed. Benzaconine does not furnish, under the several conditions tried, a monacetyl derivative, and the compounds which have been prepared containing more than one of these groups do not exhibit any of the characteristic properties of aconitine, and would seem to be isomeric, not identical; thus the triacetylbenzaconine is isomeric with diacetylaconitine, and tetracetylbenzaconine isomeric not identical with triacetylaconitine.[‡]

Aconine is the final basic product of the hydrolysis of aconitine, with which it occurs in *Aconitum napellus.*[§] It is an amorphous alkaloid readily soluble in water and in alcohol, though not in ether. Its solutions are sweet in taste, alkaline in reaction, and dextro-rotatory—like aconitine and benzaconine, aconine salts being also lævo-rotatory. The salts are crystalline; a solution of the hydrobromide has been used for the experiments described in this paper.

Adopting Wright's modified formula for aconitine, the following formulæ and names represent the alkaloids dealt with in the present paper.[||]

* Dunstan and Umney, *loc. cit.*
† Dunstan and Harrison, *ibid.*, 1893, and Dunstan and Carr, *ibid.*, 1894.
‡ Dunstan and Carr, *ibid.*, 1895.
§ Dunstan and Umney, *ibid.*, 1893.
|| See further, Dunstan, "The Nature of Aconitine" ('Pharm. Journ.,' March, 1894); and 'Collected Papers from the Research Laboratory of the Pharmaceutical Society,' vol. 2, 1896.

Aconine, $C_{24}H_{39}NO_{10}$.

Benzaconine, $C_{24}H_{36}(C_6H_5CO)NO_{10}$.

Acetylbenzaconine (aconitine), $C_{24}H_{37}(CH_3CO)(C_6H_5CO)NO_{10}$.

Diacetylaconitine, $C_{24}H_{36}(CH_3CO)_2(C_6H_5CO)NO_{10}$.

As aconitine has been so largely used by previous workers, its action will be treated of in greater detail than will be necessary when considering its derivatives or its allies obtained from other varieties of aconite.

The modes of action of aconitine, diacetylaconitine, benzaconine, and aconine, respectively, have been tested with regard to the following points :—

1. Their effect upon the blood pressure, pulse, and respiration of anæsthetised cats.

2. Their general effect, and especially their action upon temperature and respiration of rabbits, and (occasionally) of guinea-pigs.

3. Their general toxic action towards frogs with their effect in detail upon circulation, respiration, cord reflex, motility, and cutaneous sensation of these animals.

4. Their lethal dose towards some or all of the various animals employed.

Whilst the scope of this paper will be limited to these alkaloids, there are many other alkaloids and derivatives closely allied to aconitine which have been under examination, and it is intended to present a further communication concerning them with as little delay as possible. Among them may be named *pseudaconitine,* the alkaloid of *A. ferox;* *japaconitine,* the alkaloid of *A. japonicum* or *Fischeri,* as well as several derivatives of aconitine.

The following is a summary of the pharmacological action of the alkaloids.

Action on the Circulation.

Aconitine at first stimulates medullary centres slowing the heart, acceleration follows, auricles and ventricles taking up an irregular and (at one stage of toxic action) independent rhythm. Imperfect systole (especially in the ventricles) develops. Irritability of ventricular wall is much increased. Extensive variations of blood pressure accompany the preceding phenomena. After great ventricular acceleration with very imperfect systole, delirium of the ventricles supervenes. The vagus (stimulated) continues to restrain speed of contraction (especially acting upon the auricle), and may favour closer sequence of ventricular upon auricular systole, so as to cause a rise in blood pressure. For the same reason during a stage of sequence, it may cause the usual effect (fall of pressure). In slow poisoning the cardiac vagus on stimulation ceases to produce any effect. Atropine is unfavourable to the independent rhythm of auricles and ventricles

and also to the ultimate reduction of ventricular action to incoordinate contraction (delirium).

The vaso-motor centre is at first stimulated, but later depressed in function, but peripheral splanchnic stimulation is active to some degree throughout poisoning.

Diacetylaconitine, whilst producing effects in the main resembling those of aconitine, shows less tendency to cause independent rhythm of ventricles upon auricles. In half the experiments made a failure of systole occurred without asequence, in the remaining half an aconitine-like effect was witnessed.

The cardiac vagus is less affected than by aconitine, but the result of its stimulation depends much upon the sequence or non-sequence of ventricular upon auricular action present at the time.

The vaso-motor centre and peripheral vaso-constrictors respond to this alkaloid much as they do towards aconitine, but stimulation iu the early stage of action is less marked.

Benzaconine.—After very brief pulse acceleration, slowing with reduction of blood pressure occurs, the latter being due mainly to the depression of the motor mechanism within the heart. After a stage of irregularity, during which full diastole of both auricles and ventricles is exceptional, a blocking of auricular impulses to the ventricle succeeds, so that a rhythm of 2 to 1 is produced. (After aconitine this state of affairs is largely reversed.) Complete though transitory failure in the production of a spontaneous beat (the ventricles being first involved, then the auricles) is seen in a large proportion of experiments. This is not due to stimulation of inhibitory apparatus, which is put out of action by atropine, for the phenomenon occurs after atropine. It is referable rather to depression of the motor apparatus. Contraction returns spontaneously, either from spontaneous revival iu excitability of this apparatus, or from stimulation of the highly venous blood.

The vaso-motor centre, though depressed in function, retains some action, until the very low pressure is reached which invariably precedes death.

Vagus stimulation causes slowing, until in a late stage of poisoning, the blood pressure remaining very low, its action fails. After effective stimulation, pressure rises beyond the original level from acceleration of the heart and strengthening of the systole.

Digitaline is the most effective antagonist towards benzaconine.

Aconine is, relatively to the three compounds just considered, harmless towards the heart. At first it stimulates the vagal roots slightly, causing a slower beat. As, however, it strengthens the systole of the ventricle the blood-pressure rises, and is maintained at a high level throughout a long experiment. Asequence or disorder of rhythm is not produced, but an antagonistic effect is shown

towards aconitine and diacetylaconitine. In this action, aconine opposes independent rhythm of auricles and ventricles facilitating the transmission of the normal impulse, and it reduces the tendency to delirium of the ventricle. Only lethal doses reduce the activity of cardiac vagus terminations. The vaso-motor centre is practically unaffected.

The circulation remains active in frogs for days, in entire absence of reflex and respiratory movements.

Action on Respiration.

Aconitine at first stimulates the respiratory centre and the sensory vagal fibres in the lung. Depression rapidly follows, death in mammals being due to central respiratory failure. The peripheral innervation of respiratory muscles is not interfered with.

Diacetylaconitine produces a slighter initial stimulation than aconitine. Death results from central failure. Pulmonary œdema is commonly observed in rabbits. Respiratory spasm occurs at death.

Benzaconine does not appear to stimulate either respiratory centres or pulmonary vagus as do the two former. The centres are depressed from the first; respiratory failure induces death without spasm ; to this the reduced action of motor nerve endings in respiratory muscles contributes.

Aconine, whilst slowing the respiration from its action upon the centres, possesses a pronounced curare-like action upon motor nerve endings in respiratory muscles. No spasm attends death.

Action on the Nervous System.

Aconitine in large doses causes occasional loss of consciousness, with failure of conjunctival reflex and dilated pupil. This is not a directly narcotic effect, but is secondary to reduced oxidising power of the blood from circulatory and respiratory impairment. For a time there is evidence of stimulation of motor areas, and especially of the medulla with its contained centres; to this depression succeeds; reflex centres in the cord are stimulated, and then depressed by large doses. In frogs, voluntary movement outlasts reflex. Sensory nerves at the periphery are depressed in function after very transitory stimulation, whilst motor nerves are practically unaffected.

Diacetylaconitine produces a stimulation of the medulla, but less in degree than that caused by aconitine. The subsequent depression, especially of the respiratory centre, is well marked, the respiration being relatively more affected than the circulation. The general reflex function of the cord is depressed after preliminary excitement. The action with reference to sensory nerves is the same as that of

aconitine, but motor nerve terminations, though they are not power-
fully affected, are reduced in activity by diacetylaconitine.

Benzaconine causes a lethargic and ultimately semi-narcotised con-
dition, which is referable to low intracranial blood-pressure as well
as to a direct action upon the cortex. Whilst the medullary
centres are early depressed, both direct and cross cord reflexes are
elicited in a limb excluded by vascular ligature from access of the
alkaloid. Sensory nerves are unaffected except in deep poisoning.
On the other hand, motor nerves and their terminations are reduced
in function, a peculiar intermittency of response following stimula-
tion.

Aconine produces in mammals loss of volition and impairment of
conjunctival reflex only shortly before a large dose proves fatal.
Motility is interfered with, but this is mainly due to a curare-like
effect upon motor nerve endings. The respiratory centre is depressed,
respiration failing when the heart still beats vigorously.

Action on Oxidation Processes.

All the four alkaloids here considered reduce the oxidising power
of vegetable protoplasm; aconitine being most and aconine least
active. Diacetylaconitine is more energetic than benzaconine.

Action on Internal Temperature.

Aconitine produces a fall (exceptionally preceded by a slight rise),
which increases as respiratory slowing develops, but the minimum
is reached (50—70' in rabbits) after a partial recovery of respira-
tion. Exposure to a cold atmosphere increases the fall and delays
the recovery. Diminished oxidation produced directly and through
impairment of circulation and respiration indirectly are causal to the
fall.

A dose of aconitine less than half the lethal proportion will cause
a fall of nearly 2° C. below the normal.

Diacetylaconitine occasions less effect than aconitine on the tem-
perature when the dose bears an equal relationship to the respective
lethal doses. This is due to a less vigorous action on heart and
respiration. Like aconitine, it interferes both indirectly and directly
with oxidation.

Benzaconine produces a trifling reduction of temperature until a
dose is reached which greatly reduces the pulse and speed of respira-
tion when a proportionate fall occurs. The reduction of muscular
movement tends still further to limit heat production. Propor-
tionately to its toxic dose the effect is not so active as in the case of
diacetylaconitine.

Aconine is inoperative towards body temperature, except in very large doses, which enfeeble respiration and cause a curare-like action on motor nerve terminations. Even then the effect is relatively slight, as the heart remains active and the vaso-constrictor system is still in play (lethal dose causing death (guinea-pig) in 60' reduced the temperature by 1·7° C.).

Action on Skeletal Muscle.

Aconitine does not in ordinary lethal doses materially affect irritability, capacity for work, or form of contraction of frog's muscle. Exposure to the direct action of aconitine solutions causes fibrillation and lengthening of the muscle curve (an effect resembling slight veratrine action has been described). Fibrillation is abolished by aconine and curare, and is therefore not attributable to the action of aconitine directly on muscular tissue, but to a stimulation of motor nerve endings.

Diacetylaconitine reduces the irritability of muscular tissue, the muscle (after poisoning *in situ*) is more readily fatigued, the curve of contraction therefore losing in altitude whilst increasing in length.

Benzaconine in large doses produces rapid fatigue with failure of contractility which is, however, restored by rest. Contact of strong solutions reduces excitability and capacity for work.

Aconine in doses sufficient to immobilise frogs is inoperative. Larger quantities slightly reduce excitability and capacity for work, but have not the action so characteristic of benzaconine.

Lethal Doses.

The results are stated in decimals of a gram per kilo. of the body weight, and where two figures are given the lethal dose lies between them.

	Cat.	Rabbit.	Guinea pig.	Frog (R. Temp.).
Aconitine	0·000134	0·000139	0·00012	{ 0·000586 (March) 0·0014 (July)
Diacetyl-aco- } nitine .... }	0·004—0·00515	0·0042	0·0042	0·039
Benzaconine ..	0·0245	0·0272	0·0238—0·0293	0·284
Aconine	0·166—0·4	—	0·275	1·055—1·75

General Conclusions.

It would therefore appear from our study of the pharmacology of these alkaloids, that the introduction of two additional acetyl groups into the molecule of aconitine does not create any pronounced varia-

tion in the pharmacological action, but results merely in a general weakening of the characteristic action of the parent alkaloid.

Considering next the effect of removing the acetyl group from aconitine, which is seen in the behaviour of benzaconine, we find that the characteristic features of aconitine action are almost entirely annulled. The great toxic power of aconitine has been greatly reduced, so that the lethal dose of benzaconine for both cold- and warm-blooded animals is relatively so considerable as to remove it from the class of poisons in the ordinary acceptation of the term.

In the action of benzaconine on the heart and circulation very little trace of the effects of aconitine can be observed; whilst after the administration of aconitine the ventricles ultimately beat more rapidly than, and often independently of, the auricles, the opposite is the case in the action of benzaconine. On the heart, indeed, it acts to some extent as the antagonist of aconitine, causing slowing, especially of the ventricles, in opposition to the great acceleration produced by aconitine, so that in a certain measure it is observed that benzaconine behaves as an antidote to aconitine poisoning, though not so effectively as atropine. This is a point of considerable practical importance when it is remembered that benzaconine occurs to a variable extent with aconine in *A. napellus*, from which plant the ordinary medicinal preparations are made.

The removal of the acetyl group has also abolished the stimulating effect of aconitine on the respiratory centres and the pulmonary vagus. On the other hand, in its general action on the respiration and on temperature a certain resemblance is traceable between the depressant action of benzaconine and aconitine. Peripherally benzaconine depresses the activity of motor nerve endings and, in a lesser degree, of skeletal muscular tissue, whilst aconitine acts principally upon sensory nerve terminations.

In contrasting the action of aconine with that of benzaconine we are studying the effect of withdrawing a benzoyl group. It has been seen that in removing the acetyl group from aconitine we produce an alkaloid which is no longer a virulent heart poison; the removal of the benzoyl or benzoic group from benzaconine furnishes aconine which is so far from being a heart poison that it may be ranked as a general cardiac tonic, and in virtue of this action as the antagonist of aconitine. In a much greater degree than benzaconine it is an antidote to aconitine, so much so that we have found that the administration of aconine is successful in averting in small animals the effect of a lethal dose of aconitine. Amongst the distinctive features in the pharmacological action of aconine is to be noticed a curare-like effect on the motor nerve endings of the muscles which is not observed with either aconitine or diacetyl-aconitine. *No fault of sequence* between ventricles and auricles, such as is

observed (though in an opposite direction), after the administration of aconitine and benzaconine can be observed in the action of aconine. Aconine cannot be classed as a poisonous alkaloid, very large doses being necessary to produce death even in frogs.

The results of this inquiry, which has occupied the authors for the greater part of four years, brings out in a most striking manner the almost complete dependence of the extraordinary toxic power and pharmacological action of the aconitine molecule on the presence of the radical (acetyl) of acetic acid, whilst, in a lesser degree, the action of benzaconine is seen to depend on the existence in the molecule of this alkaloid of the radical (benzoyl) of benzoic acid. The inertness of the alkaloid, aconine, denuded of both the acetyl and benzoyl groups of aconitine seems to the authors to be one of the most interesting facts in chemical pharmacology. From the practical point of view the authors regard the demonstration of the antagonism of aconine and benzaconine towards aconitine as an important result of this investigation, which, taken as a whole, it is believed will throw into clearer light the mode of action of the alkaloids of *Aconitum napellus*.

The chemical part of this inquiry, for which one of us (D.) is responsible, has been conducted at first in the Research Laboratories of the Pharmaceutical Society, and afterwards in the Scientific Department of the Imperial Institute. The pharmacological experiments have been made in the Department of Pharmacology, in the University of Aberdeen.

February 10, 1898.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Contributions to the Theory of Alternating Currents." By W. G. RHODES, M.Sc. (Vict.). Communicated by ARTHUR SCHUSTER, F.R.S.

II. "The Development and Morphology of the Vascular System in Mammals. I. The Posterior End of the Aorta and the Iliac Arteries." By ALFRED H. YOUNG, M.B., F.R.C.S., and ARTHUR ROBINSON, M.D. Communicated by Sir WILLIAM TURNER, F.R.S.

III. "Further Observations upon the Comparative Chemistry of the Suprarenal Capsules, with Remarks upon the Non-existence of Suprarenal Medulla in Teleostean Fishes." By B. MOORE, M.A., Sharpey Scholar, University College, London, and SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar. Communicated by Professor E. A. SCHÄFER, F.R.S.

IV. "The Effects of Extirpation of the Suprarenal Bodies of the Eel (*Anguilla anguilla*)." By SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar. Communicated by Professor E. A. SCHÄFER, F.R.S.

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"Contributions to the Theory of Alternating Currents." By W. G. RHODES, M.Sc. (Vict.). Communicated by ARTHUR SCHUSTER, F.R.S. Received January 4,—Read February 10, 1898.

(Abstract.)

This paper is divided into two parts. Part 1 deals with a method of finding the steady values of alternating currents in any circuits or systems of circuits, without having to perform integrations of differential equations which may be somewhat complicated.

It is assumed, however, that the electromotive forces and electric

currents can be represented as simple sine (or cosine) functions of the time.

The method consists in applying the fact that if a simple harmonic function is differentiated twice in succession the result is proportional to the original function. By the application of this principle the determination of the steady values of the currents is reduced to solving a set of simultaneous *simple* equations.

After introducing the method by solving some simple problems, it is applied to the following :—

(*a*) The determination of the equivalent resistance R, reactance and impedance I, of a parallel circuit of *n* branches, taking into account mutual induction, when each branch may contain resistance, capacity, and self-induction.

The result is written

$$R = \frac{A_0 C}{C^2 + p^2 B^2}, \qquad L = \frac{A_0 B}{C^2 + p^2 B^2}, \qquad I = \frac{A_0}{\sqrt{C^2 + p^2 B^2}},$$

L being the equivalent self-induction, where A_0, C, B are certain functions of the resistances, self-inductions, capacities, and mutual inductions of the several circuits.

(*b*) The determination of the currents in the *n* circuits of an air core transformer having one primary coil and $n-1$ secondary coils.

In addition to solving the problem, the conditions for resonance in the primary circuit are obtained and discussed, and special attention is given to the case of a transformer having only one secondary coil.

(*c*) The determination of the outputs of *n* alternators working in parallel on a non-inductive external circuit.

PART II.

This part is devoted to the consideration of the effects of higher harmonics in E.M.F.s and currents on the values of the impedances and reactances of circuits.

The problems considered in Part 1 are again discussed on the assumption that the impressed potential difference is of the form

$$E = E_1 \sin (pt - \theta_1) + E_2 \sin (2pt - \theta_2) + \ldots + E_m \sin (mpt - \theta_m).$$

It is also shown that periodic E.M.F.s and corresponding currents can in all cases be represented by simple sine curves having the same root mean square values, and suitable phase positions depending on the time constants of the circuits and on the periodicities of the harmonics present.

" The Development and Morphology of the Vascular System
    in Mammals.   I. The Posterior End of the Aorta and the
    Iliac Arteries."   By ALFRED H. YOUNG, M.B., F.R.C.S., and
    ARTHUR ROBINSON, M.D.   Communicated by Sir WILLIAM
    TURNER, F.R.S.   Received January 21,—Read February 10,
    1898.

(Abstract.)

Though numerous observations have been made on the develop-
ment of the systemic aorta and on the aortic arches, including
their modifications and transformations at the head end of the embryo,
but little attention has been given to the development and modifica-
tions of the primitive vessels and the aortic arches at the caudal end.

The statement that the primitive aortæ are prolonged backwards
from the dorsal region into the tail, and that, fusing there, they form
a caudal aorta—the middle sacral artery—seems to be generally
accepted by embryologists.   Obviously, if this view is correct, the
iliac arteries are not formed from, and do not represent any part of,
the primitive aortæ, and they are generally regarded as being seg-
mental in character.

Previous observations on the comparative anatomy of the mam-
malian aorta and its terminal branches made by one of ourselves in
1891, seemed to show that the true posterior continuation of each
primitive dorsal aorta was to be found, not in the middle sacral
artery, but in the iliac and hypogastric trunks.   It was impossible,
however, to arrive at any satisfactory conclusion on the question in
the absence of definite and precise information regarding the develop-
ment of the caudal end of the aorta and its branches.

We therefore commenced a series of observations on the develop-
ment of the posterior parts of the main systemic vessels in the
rat, mouse, ferret, cat, and sheep.   In the first three of these the
sections examined represent a fairly complete series of the early
stages of development.   In the cat several embryos of two different
stages were referred to, whilst our sections of sheep embryos repre-
sented only one stage of development.

In our description of these the embryos of each class are arranged
in groups according to the stage of development of their arterial
systems, and in each group the general stage of development is indi-
cated by a short statement of the condition of some of the main
organs.

On developmental grounds alone the following conclusions, which
form a brief summary of the results of our observations, have been
arrived at :—

    1. *The primitive aortæ are paired trunks which pass at either*

end of the embryonic area into the vascular network on the wall of the yolk-sac.

2. With the formation of the cephalic and caudal folds each primitive trunk, at first almost straight, is so folded in front and behind that a dorsal and two ventral portions with uniting caudal and cephalic arches are differentiated.

3. The dorsal part of each trunk is modified in the cephalic region into vessels of the head and neck; the remainder of each dorsal portion fuses with its fellow of the opposite side to form the greater part of the systemic aorta.

4. The cephalic or anterior ventral portions are converted into the heart, the ventral part of the arch of the adult aorta, and vessels of the head and neck.

5. The caudal or posterior ventral portions either fuse together to form a common vitello-allantoic stem, as in rodents, or they remain separate and form the ventral parts of the allantoic arteries as in carnivores, ruminants, and man.

6. The ventral and dorsal sections at first are united anteriorly by a cephalic arch and posteriorly by a primary caudal arch. Additional cephalic arches are developed subsequently, to be afterwards utilised in the formation of vessels of the head, neck, and upper extremity and of part of the arch of the aorta of the adult, whilst additional arches in the caudal region may also be formed to be utilised as visceral arteries.

7. The dorsal and ventral extremities of the primary caudal arches remain; the dorsal extremities take part in the formation of the posterior end of the aorta of the adult; the ventral extremities are utilised in the formation of the ventral portions of the allantoic or hypogastric arteries.

8. The middle parts of the primary caudal arches disappear and are replaced by "secondary" caudal arches which lie to the outer sides of the Wolffian ducts. In rodents and man the secondary arches are transformed into the common and internal iliac arteries and the dorsal parts of the hypogastric arteries, whilst in carnivores they are probably transformed into the posterior part of the adult aorta and into the internal iliacs and dorsal parts of the hypogastric arteries.

9. The vessels which are to be looked upon as the posterior continuations of the primitive aorta in the adult in man, rodents, &c., are the common iliac, internal iliac, and hypogastric arteries, and in the carnivores, &c., the internal iliac and hypogastric arteries.

10. The common and internal iliac arteries are not segmental vessels, their branches may be.

11. The middle sacral artery is a secondary branch, probably representing fused segmental vessels.

12. The systemic aorta is formed from the following parts of the

primitive vessels : the anterior ventral aorta, the fourth left cephalic
aortic arch, the fused portions of the primitive dorsal aortæ, and in
some mammals the fused dorsal ends of the caudal arches.

The permanent adult aorta, in so far as it is formed by the primi-
tive dorsal aortæ, ends posteriorly either at the bifurcation into the
two common iliac arteries or at a point corresponding to this bifurca-
tion, when by more extensive fusion involving the dorsal parts of the
secondary arches there are no common iliacs, and the external and
internal iliac arteries appear to arise directly and separately from the
aortæ.   In each case the continuity of the primitive aorta is inter-
rupted, and the primary caudal arches are replaced by secondary
caudal arches, after which the continuations of the aorta are repre-
sented by the vessels into which the secondary caudal arches are
ultimately transformed.

Our conclusions are further supported by more extended observa-
tions on the anatomy of the posterior end of the aorta, and its
terminal branches in mammals, and on the abnormalities they present
in man, a general account of which is included in the memoir.

" Further Observations upon the Comparative Chemistry of
    the Suprarenal Capsules, with Remarks upon the Non-
    existence of Suprarenal Medulla in Teleostean Fishes."
    By B. MOORE, M.A., Sharpey Scholar, University College,
    London, and SWALE VINCENT, M.B. (Lond.), British Medical
    Association Research Scholar. Communicated by Professor
    E. A. SCHÄFER, F.R.S.   Received January 27,—Read
    February 10, 1898.

In a previous communication* we have shown that the paired
segmental suprarenals of Elasmobranchs contain a chromogen which
gives the same reactions as that of the medullary portion of the supra-
renal capsule of higher vertebrates, while the inter-renal body in the
same order of fishes contains no such chromogen.   These facts were
put forward in support of views previously expressed,† that the
segmental bodies corresponded physiologically, as well as morpho-
logically and histologically, to the medulla of mammalian suprarenal,
while the inter-renal corresponded to the cortex.

Now it has been already pointed out‡ that the known suprarenal

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* ' Roy. Soc. Proc.,' 1897 (read December 11, 1897).

† Swale Vincent, ' Roy. Soc. Proc.,' vol. 61, p. 64, and *ibid.*, vol. 62, p. 176, and
other references (given in these two papers).

‡ *Swale Vincent, loc. cit.*

bodies· ("corpuscles of Stannius") of Teleosts do not contain the physiologically active principle which is characteristic of suprarenal medulla. This is shown both by testing the action of an extract made from them upon the blood-pressure of a living mammal, and also by the effects of subcutaneous injection of an extract.* In both cases negative results are obtained.

The natural conclusion to be drawn from these observations would seem to be that the representative of the suprarenal medulla is absent in Teleostean fishes. But that an organ of such manifest and vital importance in mammals† should be totally unrepresented in by far the majority of living fishes, seemed to us so remarkable that we considered it necessary to furnish some further evidence upon this point.

In our previous paper upon the comparative chemistry of the suprarenal capsules,‡ we had to regret that material for investigation of the chemistry in Teleosts had been wanting. Since then, however, we have obtained six large specimens of *Gadus morrhua*. The suprarenal bodies obtained from these weighed in a moist state 0·42 gram. These were boiled with normal·saline so as to make a 10 per cent. decoction; this was carefully filtered and the pale yellow filtrate tested for the chromogen with chromic acid, ferric chloride, &c., as described in our previous paper, but *no colour reactions whatever were obtained*. The same experiment was tried with material from *Anguilla anguilla*. As some observers§ have believed the lymphoid "head-kidney" to have something to do with the suprarenal bodies, we have tested this also for the chromogen, with entirely negative results.‖

Again, we have examined other portions of the kidney with the greatest minuteness, but have failed to find anything which resembled the suprarenal medulla, either in its histological, physiological, or chemical features.

The chief facts in our possession are, then, as follows :—

1. The known suprarenal bodies of Teleosts resemble anatomically and histologically the inter-renal body of Elasmobranchs and the cortical portion of the suprarenal capsules of higher vertebrates.

2. An extract made from them, when injected into the blood-vessels of a living mammal, does not raise the blood-pressure.

3. The extract does not produce physiological effects when injected subcutaneously.

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* Swale Vincent, *loc. cit.*

† See Oliver and Schäfer, 'Journ. of Physiol,' vol. 18, No. 3, 1895.

‡ 'Roy. Soc. Proc.,' read December 11, 1897.

§ 'Weldon, 'Quart. Journ. Mic. Soc.,' vol. 24, p. 171, and vol. 25, p. 127; also Grosglik, 'Zool. Anz.,' 1885.

‖ It has been previously determined that "head-kidney" contains no phys'ologically active substance.

4. The bodies do not contain the chromogen which is always present in suprarenal medulla.

5. The lymphoid "head-kidney" presents none of the features, anatomical or histological, which would lead one to conclude it had anything to do with the suprarenal gland: moreover, extracts prepared from it have no physiological action, and contain no chromogen.

6. Other portions of the kidney give the same negative results.

7. No other gland or tissue which might be suprarenal medulla is revealed by the most careful dissection.

From these observations we are forced to the conclusion that the medullary portion of the suprarenal capsules is non-existent in Teleostean fishes.[*]

"The Effects of Extirpation of the Suprarenal Bodies of the Eel (*Anguilla anguilla*)." By SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar. Communicated by Professor E. A. SCHÄFER, F.R.S. Received February 3,—Read February 10, 1898.

(From the Physiological Laboratory, University College, London.)

Since an extract obtained from the suprarenal bodies of Teleostean fishes produces no rise of blood-pressure when injected into the blood-vessels of a living mammal,[†] and since the extract produces no physiological effects when injected subcutaneously,[‡] and, moreover, contains no chromogen, it[§] seems clear that these bodies contain nothing corresponding to the medulla of the suprarenal capsules of the higher vertebrata.[§] These results entirely corroborated the opinion previously entertained from morphological and histological considerations, that the suprarenal gland of Teleostean fishes consists entirely of cortex.[||]

Now all we know about the functions of the suprarenal capsules is confined to the medulla,[¶] and although the cortex bears every appear-

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[*] There may of course be some gland or tissue somewhere in the body which pours into the blood-stream a substance having the same physiological action as that which can be extracted from mammalian medulla, but unless this were a definite gland, and possessed a recognisable histological structure, we could not reasonably call it suprarenal medulla.

[†] Swale Vincent, 'Roy. Soc. Proc.,' vol. 61, p. 68.

[‡] Swale Vincent, 'Roy. Soc. Proc.,' vol. 62, p. 177.

[§] B. Moore and Swale Vincent, 'Roy. Soc. Proc.,' vol. 62, p. 280.

[||] Swale Vincent, 'Anat. Anz.,' vol. 14, No. 5, 1897, p. 152; see also [‡].

[¶] Oliver and Schäfer, 'Journ. of Physiol.,' vol. 18, No. 3, 1895, p. 269; Swale Vincent, 'Journ of Physiol.,' vol. 22 (Nos. 1 and 2), Sept. 1, 1897, p. 119.

ance of being an actively secreting gland, one can at present offer no
satisfactory suggestion as to the nature of its activity. It is even a
matter of surmise whether it has any functional relationship to the
medulla, considering its distinct origin and location in Elasmobranch
fishes.*

It is almost universally acknowledged that removal of the supra-
renal gland in mammals (Brown-Séquard,† Tizzoni,‡ and Oliver
and Schäfer §), and in frogs (Abelous and Langlois ||), is invariably
followed, sooner or later, by death, and that the symptoms during
life are those of extreme muscular prostration. Of course in all
these cases both cortex and medulla have been removed together, and
it would be impossible to state how far the fatal effects were due to
loss of the medullary substance, and how far to the loss of the
cortical. But Teleostean fishes, having only cortex, seemed to offer
an admirable opportunity of testing how far the cortical suprarenal
glands were essential to the life of the animal.

Among Teleosts the eel is practically the only fish available for
this purpose; since in most species the suprarenal bodies lie on the
dorsal surface of the kidney, and would be practically inaccessible
during life. Again, the length of time an eel will live out of water,
and its power of resistance to the shock of operation, render it pecu-
liarly suitable for extirpation experiments.

The eels were anæsthetised by being placed for a short time in
chloroform water. The operations were performed as aseptically as
possible, but without the use of chemical antiseptics. An incision an
inch or so in length was made to one side of the anus, reaching the
middle line in front of this aperture. The abdominal cavity being
opened, the edges of the wound were held apart by means of retrac-
tors. The gut was pushed over to one side, and the ventral surface
of the kidney laid bare. The suprarenal bodies were then picked
out with a pair of fine curved forceps. After any bleeding had been
checked, the wound was sewn up and dressed with a layer of flexible
collodion.

In three cases in which the animals survived the operation, they
have appeared quite lively soon after being put back in the tank.
One survived twenty-eight days, another sixty-four days, and a third
was killed on the 119th day. These experiments show that an eel
will survive the operation of extirpation for a very much longer

* Swale Vincent, ‘Zool. Soc. Lond. Trans.,’ vol. 14, Part III, April, 1897, pp.
52—56; also ‘Birm. Nat. Hist. and Phil. Soc. Proc.,’ 1896, vol. 10, Part I, p. 1.
† ‘Journ. de la Physiol.,’ vol. 1, 1858.
‡ ‘Ziegler's Berträge,’ vol. 6, 1889, and ‘Arch. ital. de Biol.,’ vol 10.
§ *Loc. cit.*
|| ‘Compt. Rend. de la Soc. de Biol.,’ 1891; also *ibid.*, 1892, and ‘Archives de
Physiol.,’ 1892.

time than mammals or frogs, and the difference is so striking that
one must attribute it to the absence of medulla in Teleosts, and must
assume that *the cortical gland is not absolutely essential to the life of
the animal.*   The longest time that a frog will survive removal of its
capsules is, according to Abelous and Langlois,[*] twelve or thirteen
days, and this period is shortened in the summer to forty-eight hours.
Mammals usually die in a day or two.

The validity of these experiments depends obviously upon the
fact that all suprarenal material has been actually removed at the
operation.   This has been verified in two ways.   In the first place,
previous study of the anatomy of the organs in many individuals has
shown that the suprarenals are never more than two in number.
Secondly, all three animals have been carefully dissected *post
mortem*, and no trace of suprarenal bodies has been found to be left
behind.[†]

Pettit[‡] has described a true physiological compensatory hyper-
trophy of one suprarenal in the eel after the other one has been
removed.   This indicates a secreting function for this cortical gland.
Pettit looks upon this organ in the eel as the fundamental type of the
suprarenal capsule; but this view is quite untenable in the face of
the facts that it has none of the characters of the double suprarenal
of mammals, and its removal does not cause death.

"The Kelvin Quadrant Electrometer as a Wattmeter and
    Voltmeter."   By ERNEST WILSON.   Communicated by
    Dr. J. HOPKINSON, F.R.S.   Received January 11,—Read
    January 27, 1898.

During the past seven years the author has had continued experi-
ence with the Kelvin quadrant electrometer, both in connection with
scientific research and the training of electrical engineering students
in the Siemens Laboratory, King's College, London.   This paper
embodies a good deal of the experience which he has gained with the
instrument, and he has been fortunate in that two of these instru-
ments were available.   The numbers of the instruments are 71 and
184.   The writer was therefore able to test the one as a Wattmeter,
using the other for the purpose of investigating the instantaneous
rate at which work was being done by alternate currents.   The
instrument used as a Wattmeter (No. 184) is of comparatively

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[*] *Loc. cit.*

[†] For the animal which lived 119 days this statement has been verified by Pro-
fessor Schäfer.

[‡] 'Recherches sur les Capsules Surrénales,' Thèse.   Paris (Félix Alcan), 1896.

recent construction, and differs from the other principally in the omission of the guard tube in the immediate vicinity of the needle surrounding a portion of the needle axis, and the wire connecting the needle to the acid inside the jar. The induction plate employed in the old form is done away with, and the terminals are permanently fixed to the quadrants in this new instrument, otherwise, so far as the author can see, they are identical. These instruments belong to Dr. J. Hopkinson, F.R.S., the old form being the same that he has used for many years, and in connexion with which he read a paper before the Physical Society on March 14, 1885.[*]

Verification of Clerk Maxwell's Formula.

In the paper just alluded to, it is shown that the sensibility of the instrument (No. 71) increased with the charge on the needle up to a certain point, and that for further increase of the charge on the needle the sensibility diminished. The complete explanation of this is not given, and the author believes Professors Ayrton and Perry were the first to point out that this effect is due to the portion of the guard tube in the immediate neighbourhood of the needle.

To test this point in the new instrument a Kelvin vertical electrostatic voltmeter was placed across the needle and case, and a constant electromotive force applied to the quadrants, one pair being put to the case. The jar was then charged by sparks from an electrophorus, and readings taken on the voltmeter and electrometer scale. The charge was continually increased until disruption occurred between the needle and the lantern which supports the idiostatic gauge. Up to about 2,450 volts on the needle the sensibility increased, and so far as the author could see the needle was further deflected as the charge was increased up to the point of disruption, the spot of light being then off the scale. No great care was taken with this experiment, since it was only carried out for the purpose of ascertaining if diminished sensibility could be obtained with further increased charge. The results are given in fig. 1. In Clerk Maxwell's 'Electricity and Magnetism,' vol. 1, p. 273, edition 1873, it is shown that the deflection of the needle of a quadrant electrometer should vary as $(A-B)\left(C-\dfrac{A+B}{2}\right)$, where C is the potential of the needle, and A and B the potentials of the two pairs of quadrants. In fig. 1 the E.M.F. between the quadrants was less than 1 volt, and was constant. By the formula the quotient C/θ should in this case be constant where θ is the observed deflection. It varies in arbitrary units from 0·55 to 0·11 as the value of C varies from about 550 to 2,450 volts. This is working the instrument far

Fig. 1.
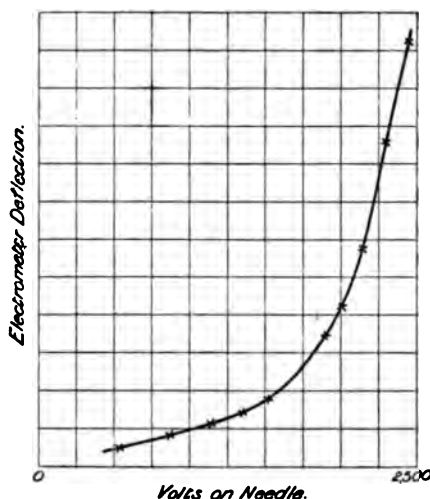
beyond the range for which it is intended, since when the gauge is in proper adjustment the value of C is only about 550 volts.

In the following experiment the highest E.M.F. employed is 115 volts, and since a square root of mean square value equal to 100 volts was the maximum potential difference about to be used by the author in a certain series of experiments upon alternate current Watt-hour meters, it was necessary to see that within this range of potential the formula above given is verified. The instrument was connected as before with one pair of quadrants to the case, the other pair being insulated and the electromotive forces applied to the quadrants, as also to the needle, were supplied by storage cells, and accurately measured by Poggendorff's method, the standard of comparison being Clark's cell. The results are given in Table I.

The instrument in the above experiment was mounted on a slate base in the upstairs room of the Siemens Laboratory. The spot of light when working on this base with this instrument is never perfectly steady, and this may account for the errors observed in Table I.

Method of Test.

Fig. 2 gives a diagram of connections showing how the electrometer was used as a Wattmeter for alternate currents, and how it was tested when being so used. In the formula

$$\theta = \lambda(A - B)\left(C - \frac{A + B}{2}\right),$$

Table I.

Observed deflection θ.	A + B volts.	C volts.	$\dfrac{\left(C - \dfrac{A + B}{2}\right)(A + B).}{\theta}$
+ 106	57·5	20·7	436
− 140	58·6	39·5	427
− 331	53·3	53·4	431
− 552	52·7	71·6	432
− 773	51·5	91·0	434
− 551	32·5	89·7	433
− 268	14·2	89·7	438
− 352	14·1	115·0	432
− 729	32·3	114·0	434
− 726	48·5	88·9	432
− 427	60·6	60·7	431
− 338	88·9	60·7	427
− 110	113 0	60·7	432
+ 626	113·0	32·2	439
+ 987	113·0	18·1	439
			433 mean
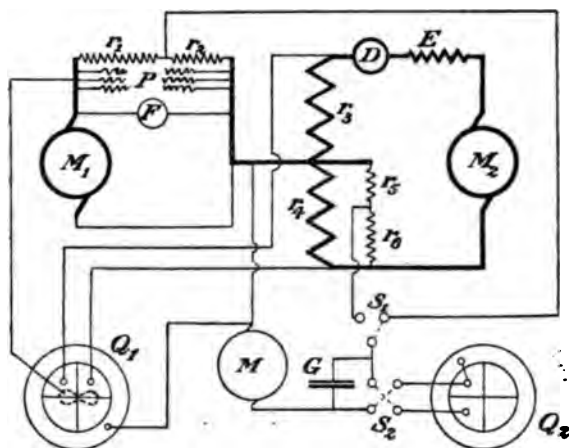
Fig. 2.

where λ is a constant, it follows that if A and B are in phase with one another and with the alternate current, and have the same wave form as the alternate current; and if C is in phase with the potential difference between two points of the circuit where the power is to be measured, and has the same wave form, A must be equal and opposite in sign to B, since the instantaneous rate at which work is done on or by the circuit must be proportional to AC or BC.

In the Siemens Laboratory there are two alternate current machines[*] coupled together in such manner that any desired phase difference between their armatures can be obtained. In fig. 2, M_1 and M_2 represent the armatures of these machines. On the shaft of one of these alternators is fixed a revolving contact maker, M, which makes contact between two brushes once in a period, that is six times in a revolution of the alternator, since there are twelve poles. It consists of a gunmetal disk keyed to the shaft of the alternator, and carrying two rings, one of ebonite and the other of gunmetal insulated from the disk by means of the ebonite ring. Into the ebonite ring are inserted six contact-making strips of gunmetal one-sixteenth of an inch thick, equally spaced out on the circumference and soldered into the gunmetal ring. An insulated copper brush bears on the gunmetal ring, and an insulated steel brush bears on the surface of the ebonite ring, touching each of the contact-making strips as the contact maker revolves. The epoch at which such contact is made by the small steel brush can be varied and observed by means of a pointer moving over a fixed circle divided into 360 equal parts. The diameter of this revolving contact maker is 13 inches.

Q_1 is the No. 184 electrometer used as a Wattmeter.

Q_2 is the No. 71 electrometer used in connection with the revolving contact maker M for the purpose of determining the instantaneous values of the current and potential difference.

D is a Kelvin balance or Siemens electro-dynamometer for the measurement of current; E is the thick wire circuit or circuits of the Watt-hour meters being tested; F is a Kelvin multicellular voltmeter; r_1, r_2 are non-inductive resistances of comparatively large value for the purpose of reducing the potential difference applied to the electrometer Q_2, when measuring potential difference C; the pressure circuits, P, of the Watt-hour meters are placed across $r_1 + r_2$; r_3, r_4 are made up of a manganin strip 50·8 mm. wide and 0·4 mm. thick; $r_3 = r_4 = 0.2275$ ohm at about 10° C.; r_5, r_6 are non-inductive resistances of considerable magnitude for reducing the potential difference applied to Q_2 when necessary. The junction between r_3 and r_4 is connected to the case of Q_1; the quadrants of this instrument are connected respectively to the extreme ends of r_3, r_4; whilst the needle of the electrometer is connected to the other pole of M_1. In connection with Q_2, S_1 is a two-way switch for observing potentials across r_3 or r_5; S_2 is the ordinary switch supplied with the electrometer which short-circuits the quadrants when moved to its central position, and in its two other positions reverses the charge on the quadrants; G is a condenser, which can be varied

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[*] A full description of these machines is given in the 'Phil. Trans.,' A, vol. 187 (1896), p. 231.

from 0·001 to 1 microfarad, its capacity being 1 microfarad during the experiments, the results of which are given in Table II.

Before giving the results of the experiments it is well to explain the method adopted of treating the curves for the purpose of arriving at the average Watts due to the alternate current, the relation between which and the deflection of the electrometer used as a Wattmeter it is desired to find. It is also necessary to examine the limits of accuracy obtainable by this method. In any one experiment the frequency employed is kept constant as nearly as possible : the phase difference between current and potential is adjusted to any desired value and the amplitude of these quantities is kept constant by observing their square root of mean square values on the instruments D and F. The revolving contact maker M is then set to different positions of the phase, the number employed being at least ten equal divisions to the half period, and for each position, readings taken on the electrometer Q_2 when the switch S_1 is in each of its two positions. If the deflections so obtained be plotted in terms of the position of the revolving contact maker M, the forms of the two curves are those due to the instantaneous values of the potential difference applied to the needle of the electrometer Q_1, and the current which gives the form of potential difference applied to the quadrants of Q_1. By multiplying each of these deflections together, and by a suitable constant involving the square of the sensibility of Q_2 and the resistances r_1, r_2; r_3, or r_3 ; r_5, r_6, the instantaneous rate at which work is being done by the alternate current can be inferred in Watts. The average of these over a half period gives the average rate, and this can be obtained by plotting the instantaneous product and taking the area with a planimeter, or the average of the algebraic sum during a half period can be taken. The author found the latter method agreed so well with the former when the number of intervals at which observations are taken is ten, that he has adopted it in this paper, that is to say, the two electrometer deflections for a given position of M are multiplied together, the average of these taken over half a period, and such average multiplied by a constant to reduce to Watts.

The best way, perhaps, to test the limits of accuracy is to adjust current and potential until they are exactly in phase. The voltmeter F and amperemeter D give the square root of mean square values, and the product of these should agree with the average results obtained from the curves. The time required to take one set of observations is generally about twenty minutes, during this time an average for volts, amperes, and frequency is taken. The author finds from experience that if care be taken an agreement between the results got from the curves, and from the product of volts and amperes, can be obtained to within one or two per cent. It

must be remembered that for each position of the contact maker, *four* observations on the electrometer (Q_2) scale have to be obtained; that is, two for potential and two for current corresponding to the two positions of S_2 for each position of S_1, the difference in each case giving the net double deflection. This method is best, as it eliminates any zero error there may be. In working the electrometer Q_2, a wooden tapper or mallet is employed, since in every electrometer there must be viscosity due to the fluid, and by gently tapping the slate base for each deflection very consistent results can be obtained. This viscosity is greater in winter, and it is advisable to keep the instrument in a warm room, although with this method of tapping the author does not find this necessary. The greatest trouble in the use of the electrometer undoubtedly arises from dust settling on the surface of the acid in the jar, thereby making the angular movement of the wire hanging from the needle smaller than it would be if such brake action did not exist. This takes place when the acid in the jar is old, and if the surface be agitated by blowing through a glass tube near where the wire dips into the acid it can be to a great extent remedied. Whatever the state of the acid the author finds he gets the most consistent results by gentle tapping. The electrometer Q_1 is not so sensitive as the old form Q_2, and the effect due to the acid in it has not given so much trouble. The sensibility of Q_2 when the idiostatic gauge is adjusted is such that one Clark cell gives a deflection from zero of $10\frac{1}{2}$ inches on a scale 12 feet from the mirror. The potential of the needle is in this case about 350 volts.

Experimental Results.

In making a thorough test of the electrometer as an alternate current Wattmeter we have the following variables to deal with :—

1. The frequency of the alternate current.
2. The phase difference between current and potential, that is between C and A or B.
3. The amplitude of C and A or B.
4. The shape or wave form of the curve of potential and current.

The results obtained are tabulated in Table II and are divided into three groups (*a*) (*b*) (*c*). In group (*a*) two frequencies are given, namely 41·6 and 83 complete periods per second. The potential on the needle is constant at about 100 volts ($\sqrt{\text{mean}^2}$). The phase difference between potential and current and the current itself are each varied. When the phase difference is zero, it is only necessary to take the product of the $\sqrt{\text{mean}^2}$ values to deduce the Watts, although in section (*b*) three instances are given in which for phase difference zero, the Watts are deduced by both methods. The

average Watts per division given by section (*a*) are 17·00 for all angles of phase leaving out the two values deduced by aid of the cosine law for angles of 30° and 60°. It will be seen that under the conditions of section (*a*) the Wattmeter may be said to be verified within the limits of accuracy attainable by the method of test. The wave form of the unloaded alternator is given in fig. 3 and marked C; this is the wave form of potential applied to the needle in all experiments in sections (*a*) and (*b*). A sine curve having the same maximum ordinate is superposed for the purpose of comparison. The current curve has different wave form according to the load on the alternator. For small currents it approximates to C in fig. 3. The curve A, fig. 3, is the wave form for current 74 amperes, which is the maximum we have employed.

FIG. 3.
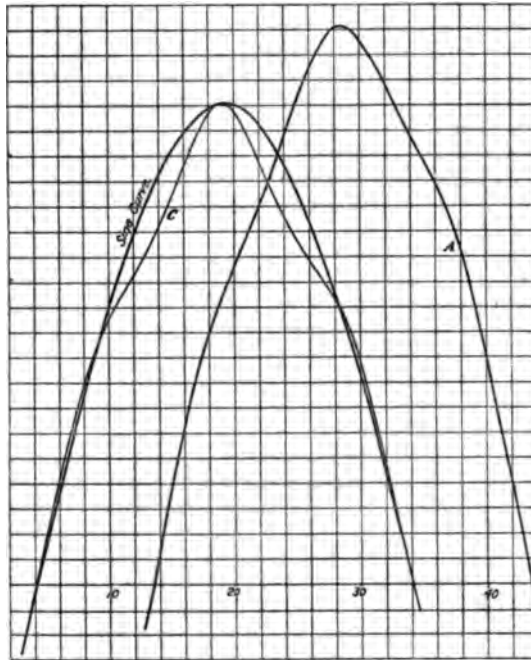
The experiments in section (*b*), Table II, are intended to demonstrate the reliability of the instrument when the potential of the needle C is varied through wide limits. One would expect from the curve in fig. 1, that for high potentials on the needle the Watts, per division of the scale, would diminish. This is found to be the case when the potential C is raised to 1,860 volts ($\sqrt{\text{mean}^2}$) for fre-

Table II.

Frequency.	Phase difference in degrees. 360° = 1 period.	Potential of needle C in $\sqrt{\text{mean}^2}$ volts.	Current in amperes $\sqrt{\text{mean}^2}$.	Watts. From product of volts and amperes.	Watts. Given by curves.	Watts per division of scale. From product of volts and amperes.	Watts per division of scale. Given by curves.	Date of experiment, 1897.
(a)								
41·6	0·0	100·35	39·65	8979·0	...	16·79	...	November 24
41·6	0·3	99·85	17·11	1709·0	...	16·75	...	,, ,, 25
41·6	0·0	100·4	39·8	8996·0	...	16·79	16·85	,, 23
41·6	29·7	100·3	39·85	8996·0	3371·0	...	17·30	,, ,, 25
41·5	31·5	99·66	16·85	...	1457·0	17·23*	...	...
41·6	30·0	100·0	39·6	...	...	18·54*	...	...
41·6	59·4	100·0	39·7	3897·0	...	16·87	...	,, 30
83·0	0·0	99·3	39·24	2991·0	...	16·85	...	December 1
83·0	0·0	100·0	29·91	980·6	...	16·90	...	,, ,, 2
83·0	0·0	101·7	9·64	984·5	...	17·27	...	...
83·0	0·0	101·5	9·70	3925·0	...	16·85	...	...
83·0	0·0	99·5	39·45	7472·0	...	16·68	...	,, ,, 1
83·0	0·0	101·0	73·98	1968·0	...	...	...	...
83·0	61·9	99·6	19·76	...	892·3	...	17·5	...
83·0	39·3	99·4	40·15	...	3191·0	...	17·34	...
83·0	68·9	100·0	39·9	...	1622·0	...	17·07	November 30
83·0	60·0	101·7	74·15	...	4097·0	...	17·29	December 4
(b)								
82·0	1·2	563·0	11·89	6689·0	6788·0	15·67	15·77	December 4

* Watts deduced by Cosine Law.

Table II—*continued.*

Frequency.	Phase difference in degrees. 360° = 1 period.	Potential of needle C in √mean² volts.	Current in amperes √mean².	Watts.		Watts per division of scale.		Date of experiment, 1897.
				From product of volts and amperes.	Given by curves.	From product of volts and amperes.	Given by curves.	
83·0	60·0	553·0	11·83	..	3142·0	..	15·25	December 4
83·2	0·8	630·0	10·86	6842·0	6992·0	16·25	16·37	,, 11
83·0	65·4	628·0	10·3	..	2433·0	..	15·7	,, 18
75·0	0·0	1860·0	2·33	4330·0	..	10·93	..	,, ,,
75·0	0·0	1860·0	1·25	2320·0	..	11·40	..	,, 9
53·6	0·0	438·0	16·03	7020·0	7162·0	16·52	16·85	,, ,,
53·7	64·8	436·0	15·01	..	2868·0	..	16·49	,, 17
43·0	0·0	1840·0	2·39	4400·0	..	11·21	..	,, ,,
43·0	0·0	1840·0	1·30	2390·0	..	11·60	..	
(c) 50·4	6·0	117·2	45·0	5275·0	5850·0	18·91	17·15	December 15
50·4	36·0	117·8	45·0	..	2741·0	..	16·82	,, ,,

quencies of 75 and 43.   In section (c) the wave form is **very much** distorted.   The curves of potential and current are plotted in fig. 4.

FIG. 4.
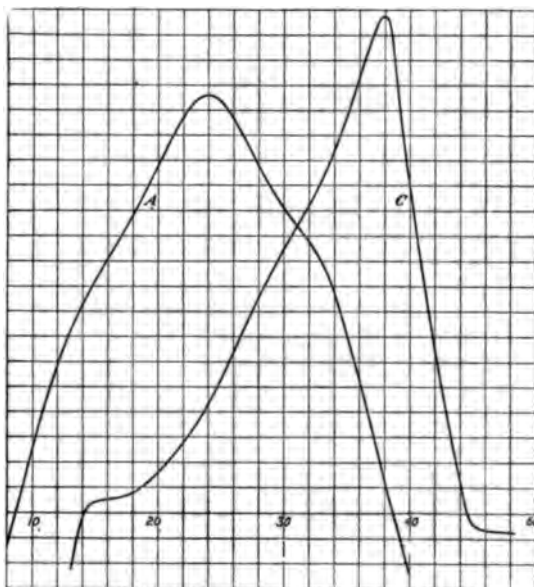
The distortion of the potential curve C was brought about by placing a considerable non-inductive resistance in series with a choking coil, and taking potentials across the choking coil.   The instrument under these conditions gives trustworthy results.   The phase difference on one or two occasions was such that the curves indicated no work, the deflection under these conditions was zero. The maximum angle of deflection of the needle from its normal position was 7·8°, and tests were made from time to time, especially with the large potentials on the needle, to see if the instrument was in proper adjustment, by placing both pairs of quadrants in con- nection with the case, and noting the agreement between its then zero and the zero when quadrants and needle were put to the case, that is when the instrument was totally discharged.   To test the effect of dismounting the instrument the needle was taken off the suspension and the instrument moved to another room and used for another purpose, on December 10, 1897.   On continuing the experi- ments it was set up by the level only, and found to be in proper adjustment.   The results of experiment before and after this *removal are given* in Table II.

Seeing from fig. 3 how great was the deviation from the sine law, it would have been necessary to analyse each curve by Fourier's theorem, if the subject was to have been treated mathematically, the phase difference being given. The current curve was continually changing its form with different loads, and this would have necessitated observing the curve in each case, so that nothing was to be gained by this method of treatment. The potential curve C, fig. 3, has, however, been analysed,[*] and can be expressed by the equation—

$$C = B_1 \sin \frac{2\pi t}{T} + B_3 \sin \frac{6\pi t}{T} + \ldots$$

The first five co-efficients are as follows :—

B_1	B_3	B_5	B_7	B_9
540·1	1·9	31·5	−6·5	1·3

We see that B_5 is important, being about 6 per cent. of B_1; so that from the analysis the cosine law could not be expected to hold. In section (a), Table II, the cosine law is applied in two instances for the purpose of illustration. It gives 18·54 as against 17·0 for the Watts, per division of scale, for 60°; and 17·2 as against 17 for 30°. For small angles the error does not appear to be so great.

The conclusions arrived at from these experiments are that the Kelvin Quadrant Electrometer can be used with accuracy as a Wattmeter in the case of alternate currents having any phase relation, and that, as pointed out by Dr. J. Hopkinson,[†] it is necessary to see that within the range of potentials applied, Maxwell's formula is verified. This is, perhaps, best done by applying steady potential differences to the needle and quadrants, and measuring these by Poggendorff's method, employing Clark's standard cell as the unit of comparison. It could also be tested by applying known alternating potentials to the needle and quadrants, the curves being in phase. If it is required to use alternating potentials of high value, such, for instance, as 2000 volts or more, a suitable transformer could be employed to reduce the potential on the needle. Such unloaded transformer could have the primary and secondary electromotive forces in phase, and of the same wave form,[‡] so that no error would be thereby introduced.

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[*] 'Electrician,' August 31, 1894, p. 517.
[†] 'Philosophical Magazine,' April, 1885.
[‡] 'Electrician,' February 15, 1895, p. 463.

The Revolving Contact Maker.

The revolving contact maker M, fig. 2, exhibits a peculiarity worth noting. It is in itself the seat of an electromotive force, as is demonstrated by placing it across the electrometer Q_2, and running the machines without excitation. A deflection of 68 scale divisions, corresponding to 0·45 volt, is given if the electrometer has no capacity across its terminals, that is, if G is zero. A copper brush gives the same effect as a steel one. As soon as G is given a substantial value as compared with the electrometer itself, this deflection disappears.

When actually observing potentials in the usual way, let the value of G be varied. For a given position of the contact maker the deflection varied, as shown in fig. 5, in which the ordinates are observed
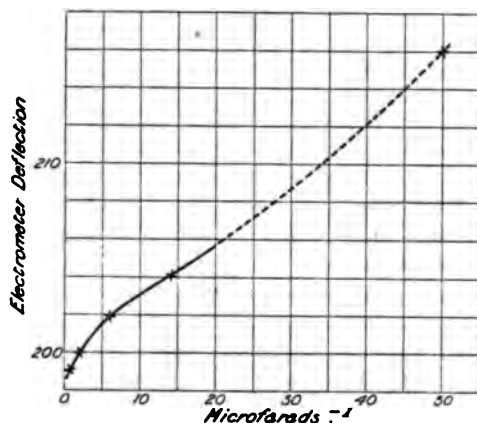
deflections, and the abscissæ the reciprocals of the capacity of G in microfarads. We see that when G has 1 microfarad capacity, the deflection is practically what it would be if G were ∞, and with 1 microfarad the results verify with the true value. Such inductive effect is certainly rendered negligible by sufficient capacity, and it is therefore wise to examine this effect when working with a given contact maker, since each one may have its own peculiarities.

The Manganin Strip.

The manganin strip r_3, r_4, fig. 2, is in lengths of 5 feet, brazed together. This material has altered its resistance, as shown in *Table III.*

Table III.

Date.	Resistance at atmospheric temperature in ohms.
1st November, 1897	0 ·4625
3rd ,, ,, ...	0 ·4590
13th ,, ,, ...	0 ·4592
20th ,, ,, ...	0 ·4589
,, ,, ,, ...	$r_3 = 0 ·2269$
,, ,, ,, ...	$r_4 = 0 ·2270$
4th January, 1898	$r_3 = 0 ·2275$
,, ,, ,, ...	$r_4 = 0 ·2277$

The strip was mounted on November 1, 1897, and submitted to currents varying from 100 amperes downwards. On November 20, 1897, it was adjusted for r_3, r_4. The results show that there is an initial diminution of resistance, and that then the resistance remains practically constant. This is worth noting, as this material is largely used at the present time, on account of its low temperature coefficient. The manganin strip is unvarnished and exposed to the atmosphere of the engine room. The conditions are therefore not the best to secure constancy of resistance, but in all probability the initial diminution is due to the brazing.

Messrs. C. J. Evans and H. H. Hodd have given me valuable assistance, not only in the experimental part of this paper, but also in the working out of the results. Messrs. Simpson, Greenbank, and Davey, the present Student Demonstrators in the Siemens Laboratory, have also helped me. I wish to acknowledge this, and to tender my thanks to these gentlemen.

"The Magnetic Properties of almost pure Iron." By ERNEST WILSON. Communicated by Dr. J. HOPKINSON, F.R.S. Received January 11,—Read January 27, 1898.

One of the two rings of almost pure iron supplied by Colonel Dyer, of the Elswick Works, to Sir Frederick Abel, K.C.B., F.R.S., by whom they were sent to Dr. John Hopkinson, F.R.S., has already formed the subject of a communication,[*] and is herein referred to as Pure Iron I. As this pure iron has not been directly tested for dissipation of energy due to magnetic hysteresis, and the second ring was available, the author thought it would be interesting to examine

* 'Roy. Soc. Proc.,' vol. 52, p. 228.

its magnetic properties: it is referred to as Pure Iron II. The substances other than iron in this specimen are stated to be—

Carbon.	Silicon.	Phosphorus.	Sulphur.	Manganese.
Trace	Trace	None	0·013	0·1

This ring has an internal diameter of 3·2 cm., an external diameter of 4·5 cm., a depth of 2·6 cm., and is wound with sixty-one turns for the secondary coil next the iron, and forty-nine turns for the primary or magnetising coils. The method of test* employs a ballistic galvanometer, and is that in use in the Siemens Laboratory, King's College, London, where the present experiments were carried out. The currents in the primary circuit were supplied by storage cells and measured by balancing the potential difference due to such currents in a standard resistance against a Clark's cell. The current meter in the circuit was only used for convenience of adjustment.

Quoting from the communication above referred to, Pure Iron I gives the following induction curve at atmospheric temperature:—

B	34	118	467	2,700	7,060	10,980	14,160	15,590	16,570	17,120	17,410
H	0·15	0·38	0·60	1·06	2·11	3·77	7·48	13·36	23·25	33·65	44·66

Pure Iron II has been tested under two conditions: (a) as received and (b) after careful annealing. The results are given in Table I, which also contains the results obtained by Professor Ewing from a sample of transformer plate rolled from Swedish iron.† The figures of Professor Ewing relating to magnetic hysteresis are exceptionally low, and although annealing has considerably improved the Pure Iron II, it is still slightly inferior to the transformer plate. On the other hand, the permeability μ of this pure iron after annealing is exceptionally high, having a value 5490 for B = 9000. The coercive force for maximum B = 15,270 is 1·13 C.G.S. units.

The figures in Table I relating to Pure Iron II after annealing have been obtained by interpolation from the actual observed data given in Table II. An induction density of 15,270 for H = 9·24 is higher than the author remembers having seen. In fact, for values of H below about 10 or 12 this specimen is exceptionally good, as is shown by the very high permeability.

* 'Roy. Soc. Proc.,' vol. 53, p. 352.
† 'Proceedings Institution of Civil Engineers,' vol. 126, p. 185.

Table I.

Limits of B in C.G.S. units per square centimetre.	Dissipation of energy by magnetic hysteresis in ergs per cycle per cubic centimetre.					
	Transformer plate rolled from Swedish iron (Ewing).		Pure Iron II tested as received.		Pure Iron II tested after annealing.	
		μ.		μ.		μ.
2000	220	2560	350	2000	262	2500
3000	410	3340	500	2730	460	3190
4000	640	3880	800	3330	720	3810
5000	910	4230	1100	3700	1010	4350
6000	1200	4410	1450	4138	1350	4800
7000	1520	4450	1760	4375	1670	5380
8000	1900	4330	2160	4445	2020	5440
9000	2310	4090	2600	4615	2450	5490
10000		3790	3100	4545	2860	5460
12000			4400	4000		4900
14000			5900	2641		3260
15000				1415		2050

Table II.

Limits of H.	Limits of B.	$\frac{1}{4\pi}\int HdB.$	μ.	Coercive force in C.G.S. units.
0·783	1965	262	2510	0·50
1·14	4840	..	4245	..
1·17	5150	1080	4400	0·73
1·42	7500	..	5280	..
1·66	9100	2490	5480	0·90
2·23	11460	..	5140	..
2·68	12500	..	4660	..
4·74	14270	..	3010	..
9·24	15270	..	1650	1·13

Apparent Magnetic Instability.

Whilst making the foregoing experiments, the author noticed how great was the apparent magnetic instability in this specimen, and thought it worth while to investigate this more closely.

It has already been noticed, and is well known, that if the magnetising force be varied from one maximum value through zero to a value equal, say, to the then coercive force of the material, that tapping the specimen will produce a considerable change of induction; or, if the observed kick on a ballistic galvanometer (in circuit with a secondary coil wound on the specimen) due to such change be added to the observed kick when the magnetising force is raised to

the opposite maximum, the sum does not equal the whole kick which would be observed if the force were at once varied from the one maximum to the other. During the interval the magnetism appears to continue to settle down, so that the change which lastly takes place is not so great as it would be if such apparent settling down did not occur.

Experiments were made to investigate the effect when the limits of B were (a) large and (b) small. It is assumed that the instrument gives the true time integral of current.

(a) Maximum B = 15,270, coercive force 1·13 C.G.S. units. The maximum force H of 9·24 C.G.S. units was suddenly varied through zero to 1·13, and the secondary circuit kept closed until deflections to the left and right were observed, the periodic time of the galvanometer needle being 10·6 seconds. The scale is graduated from 0 on the left to 1000 on the right, and the readings taken were 351, 623, giving a difference of 272, corresponding to a change of induction per square centimetre of 12,630 C.G.S. units. When the magnetism had settled down, as was shown by closing the secondary key with no extra resistance in its circuit, and observing no deflection on the ballistic galvanometer, a suitable extra resistance was inserted, and the force suddenly raised to its maximum value, the observed deflections were 362, 627, the difference 265 corresponding to B = 12,350. These results were many times repeated.

The total change of induction produced a deflection 662, 330, the difference 332 corresponding to B = 15,270. We have therefore to account for a difference of 5560, or 18 per cent. of the total change from one maximum to the other. The zero, when the spot of light is perfectly steady, is 495, and we can see that when making the first change from one maximum through zero to force 1·13 the deflection to the left is 143 as against 128 to the right; whereas when making the second change the deflections are 133 to the left and 132 to right. There is evidence here of a change continuing in the same direction, since the first elongation is greater than the second, and the decrement would only account for about 1 per cent.

This effect was next observed in a slightly different manner. The change of force from one maximum through zero to the then coercive force was effected, and the secondary circuit closed at known intervals of time after such change. The results are given in Table III.

It will be seen from the figures that about 30 per cent. comes out after the first second has elapsed, and that the result is practically the same, whether the charging potential difference be that due to ten or fifty-six cells. With a total reversal from one maximum to the other no such effect was observed, the change taking place *immediately*. Having taken the force from one maximum through

Table III.

Time in seconds	0	1	2	3	4	5	6	10
Change of B, 10 cells exciting through extra resistance	13600	4030	1990	927	576	285	175	42
Change of B, 56 cells exciting through extra resistance	13800	3690	1680	944	529	256	..	40

zero to a value equal to the then coercive force, the specimen was tapped four times with a piece of wood, and at each stroke it delivered 105, 40, 56, 30 C.G.S. units per square centimetre in the direction of acquirement of magnetism.

(b) Maximum B = 3770, maximum H = 1·003. The force was varied from one maximum through zero to 0·620 and a deflection corresponding to B = 3620 observed. The figures in Table IV give the results obtained by closing the secondary circuit at known intervals of time after reversal.

Table IV.

Time in seconds	0	1	2	3	5
Change of B, 5 cells exciting through extra resistance	3620	536	95	25	10

One would expect the maximum induction to be affected in this case, since it is on the steep part of the curve: the figures obtained are given in Table V.

Table V.

Time in seconds	0	1	2	3	5
Change of B, 5 cells exciting through extra resistance	3770	796	23	14	6·5

The curve in fig. 1 shows this effect clearly; the points 1, 2, 3, for any force show the observed change of induction density B when the secondary key is raised ½ second, 1 second after reversal, and

Mr. E. Wilson.
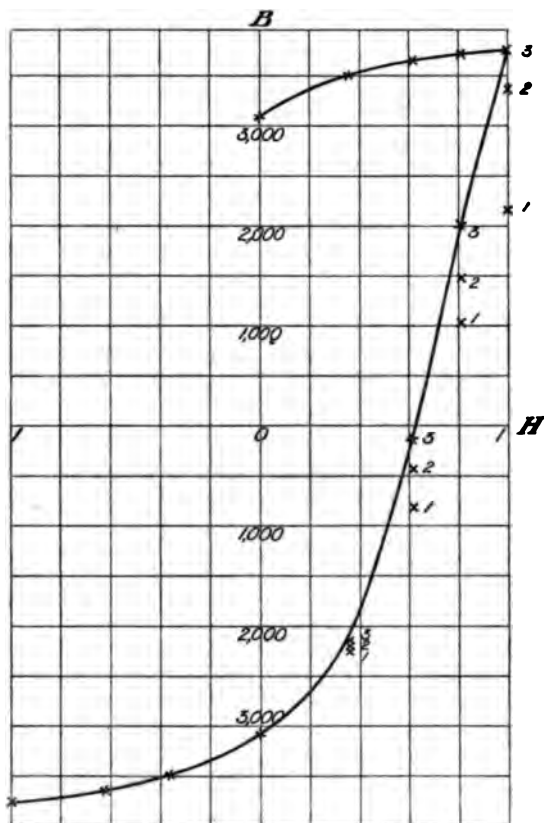
FIG. 1.

kept down permanently as in the ordinary way. When observing the deflections for the curve in fig. 1, the five cells used for exciting had placed across their terminals a condenser of 4 microfarads capacity.

We are dealing with a very steep curve in these experiments, that is to say, the rising portion for the large forces is very nearly perpendicular. We observe that it is on the steep portions that these effects have been noticed, and such effects could very easily be produced by a slow change in the magnetising force. Such slow change might arise from the heating of resistances in the circuit if these be of carbon; this was looked into and only metal resistances used. The self-induction of the circuit might, if large enough, delay the magnetising current and produce the effect. We have seen that it is practically the same whether the applied potential be that due to *ten or fifty-six cells*. In any case the self-induction can be approxi-

mately calculated in our case. Take the curve in fig. 1—we see B increases 5750, whilst H increases 0·6. The total change is 9860, since the cross sectional area of the specimen is 1·715 sq. cm. H = 0·6 corresponds to a change of 0·117 ampere, and taking the volt as our unit and the primary turns at 50, we find L = 4·21 × 10⁻², where L is the coefficient of self-induction. If E be the applied potential and R the resistance of the circuit, the current at any time t after closing the circuit can be expressed by $\frac{E}{R}(1-\epsilon^{-\frac{R}{L}t})$. For $t = \frac{1}{2}$ second the current has its maximum value within an exceedingly small quantity. But the method of experiment enabled one to test the rapidity with which the current rises to its maximum value. Let a balance be made, say when the current is such that the force is equal to the coercive force of the material. Now suddenly reverse the force from its maximum through zero to this value. The immediate depression of a key tells at once if the current is still balanced. The current immediately after reversal, that is to say within $\frac{1}{4}$ second, had certainly attained its normal value to within 0·3 or 0·4 per cent.

The condenser had no material effect upon the rapidity with which the current attained its maximum value. We can only conclude that the effect is peculiar to the iron itself, and might be influenced by induced currents, since the ring is not subdivided. The subject of propagation of magnetism as affected by induced currents has been dealt with in the case of a magnet having a core 12 inches diameter,[*] and a magnet having a diameter of 4 inches.[†] Imagine that the cross-section of our pure iron specimen is circular instead of rectangular—it would have a diameter of 14·8 mm. If we assume equal conductivities and magnetic properties, we can infer roughly from the 12- and 4-inch magnets what the effect of induced currents in our specimen would be.

Take the 12-inch magnet. For reversal of maximum H = 2·4 the effects had died away in about 400 seconds. Similar events will happen in the pure iron core, but at times varying as $\left(\frac{14·8}{305}\right)^2$: that is, we should expect the effects to have subsided in 0·85 second.

Take the 4-inch magnet. For reversal of magnetism H = 1·7 the effects had subsided in about 40 seconds: $\left(\frac{14·8}{101·6}\right)^2 \times 40$ gives 0·94 second.

It is, therefore, probable that the induced currents in the pure Iron II may have something to do with the effects observed in this paper, although it is difficult to account for such times as 5 and

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[*] 'Journal of the Institution of Electrical Engineers,' vol. 24, No. 116 (1895).

[†] 'Phil. Trans.,' A, vol. 186, pp. 93—121.

10 seconds, unless the molecule itself is considered.   This effect has been observed in laminated specimens.

The difficulty of working with alternate currents, if the core be subdivided, in order to investigate the effects observed, using the method in the 'Proceedings of the Royal Society,' vol. 53, p. 352, is the necessity for the very accurate control and measurement of the magnetising force.   Small variations of this force would at once mask the effects observed.   In the paper just mentioned a considerable difference was observed between cyclic curves obtained with the ballistic galvanometer and by means of alternate currents having frequencies of 72 and 125 per second in the case of a laminated hard steel ring for maximum $B = 16,000$.   On the other hand, no such difference was observed in the case of a laminated soft iron ring when maximum B was 4000.[*]   It would seem from the experiments in this paper that the amplitude of induction would not be so great for high frequency and small induction density B, and this is of import-ance in the case of iron cores for transformers.   It is worth noting that when working on solid rings with the ballistic galvanometer induced currents may account for apparent magnetic instability.

Mr. H. H. Hodd has helped me in the experimental part of this paper, and I here wish to tender him my thanks.

"On a new Method of Determining the Vapour Pressures of Solutions."   By E. B. H. WADE, B.A.   Communicated by Professor J. J. THOMSON, F.R.S.   Read May 13, 1897.

(Amplified Abstract, received December 22, 1897.)

On a previous occasion[†] I gave some boiling points of salt solu-tions under atmospheric pressure.   As the dimensions of that abstract made a full account of the experimental method impossible, I have been given this opportunity, by the courtesy of the Council of the Royal Society, of describing the apparatus and procedure by which those results were obtained.

§ 1. *Difficulties to be overcome.*

The exact determination of boiling points of solutions has been attended hitherto with a good deal of difficulty.   The boiling point of the pure solvent is first determined.   Salt is then added, and the boiling point is redetermined.   The experiment consists, in fact, of two parts, and the difficulty lies in making the circumstances in which the first part of the experiment was carried out identical with

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[*] See 'Electrician,' September 9, 1892.

[†] 'Roy. Soc. Proc.,' vol. 61, pp. 285—287.

those in the second. Examples of such circumstances are, barometric pressure, rate of ebullition, height of flame employed, depth of experimental liquid, and many others, and there is no doubt that the chief reason why the boiling-point method has been so little utilised, is the inability to reproduce these conditions. In particular, if the pressure under which the boiling point is to be measured is much less than 760 mm., the difficulties are so much increased that no one seems even to have attempted to make observations under such conditions.

§ 2. *Principle on which success depends in measuring by the Boiling-Point Method the Vapour Pressure of Salt Solutions.*

The difficulty in applying and extending the boiling-point method disappears in a great measure if not entirely, if the apparatus is duplicated, *and the difference alone is measured of pure water and solution boiling under similar conditions and under the same pressure, the magnitude of which need not then be known to within a millimetre.*

The difference of temperature referred to may very profitably be measured by the platinum resistance method. A decided advantage of methods which obey the principle above stated, lies in the fact that, whereas the differences of vapour pressure, measured at a common temperature, diminish very rapidly as the temperature is lowered, the difference of temperatures, measured under a common pressure, varies much less rapidly. As an example, suppose that a certain solution under a pressure of 760 mm. has a boiling point 0·52° higher than pure water, then under 360 mm. it will be found to have a boiling point 0·45° higher than that of pure water, a diminution of only 14 per cent. The corresponding diminution in the difference of tension would be 53 per cent.

The first apparatus employed by me to take the difference of boiling points of pure and salt water was in form very simple, and constructed from ordinary laboratory apparatus. A description of it is here introduced, mainly because it illustrates the principle of the more complex apparatus shortly to be described.

A current of steam from a boiler divides itself at A, fig. 1, and passes through two precisely similar pieces of apparatus, one containing pure, the other containing salt, water. The contents are agitated by the steam, and so raised to their boiling points, and the difference alone of the temperatures in the liquids was found by the two platinum thermometers T_1 T_2. Without giving details of this measurement it may at once be said that, as soon as it was complete, solution could be drawn off at the tap t_1 and its concentration determined. The remaining taps (t_2, t_3) serve to regulate the flow of steam, and the bulbs to prevent a suck-back. The apparatus worked

Fig. 1.
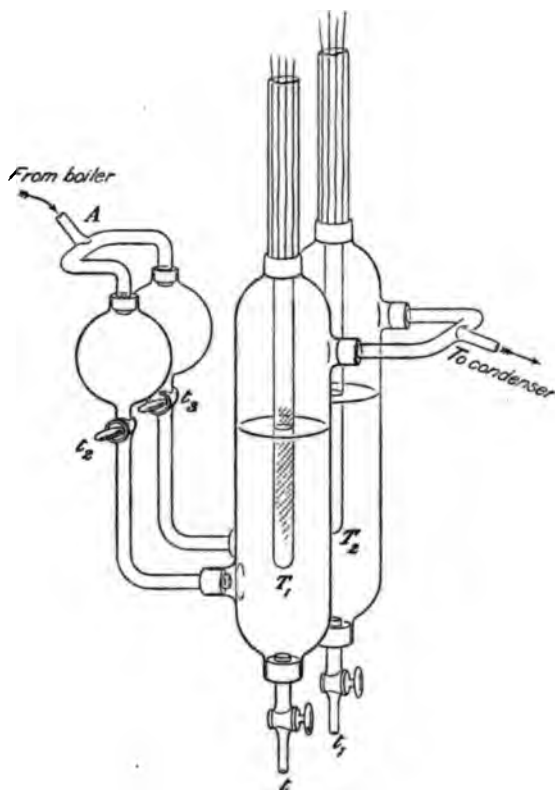
well, but the condensation was so rapid that the contents of the U-tubes accumulating became unmanageable.   The difficulty was only partially overcome by jacketing with flannel.   The trouble was all the greater, inasmuch as the estimation of temperature by the platinum method requires some little time.   The only possible way of obviating this condensation was to employ a steam-jacket, and in order to do so the apparatus was completely reconstructed, so that it assumed the form described in the next section.

§ 3. *The Apparatus employed.*

The apparatus which is about to be described is that which yielded the experimental numbers.[*]   Although its principle is fully contained in the very simple model of the last section, yet its form is somewhat complicated.

[*] 'Roy. Soc. Proc.,' vol. 61, pp. 285—287.

(PLATE 1.)
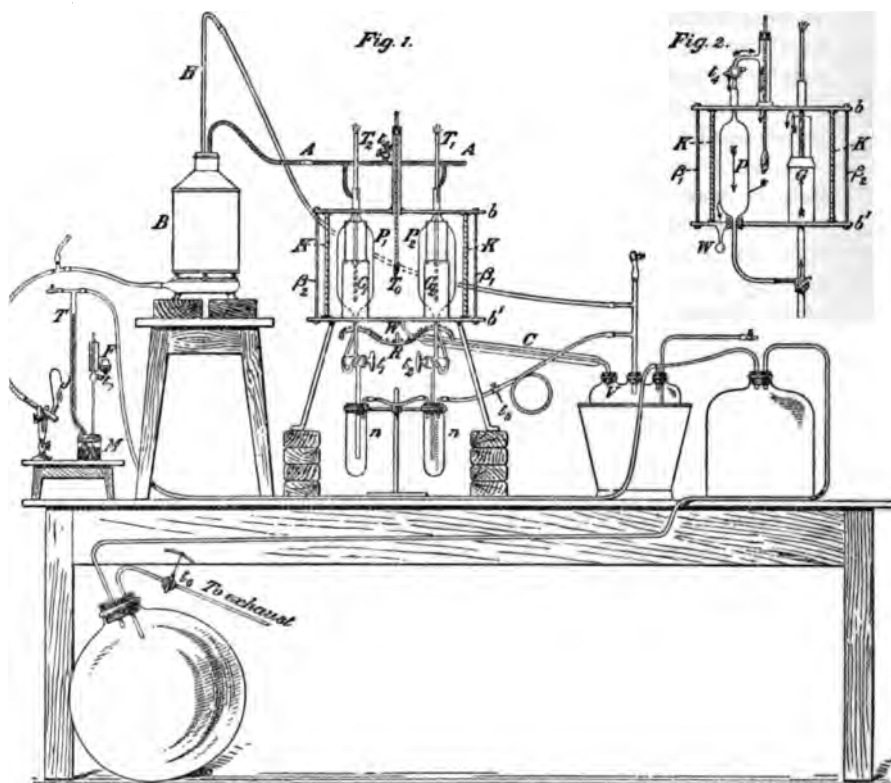
The essential part of the apparatus is as follows: $b\,b'$ (Plate 1, figs. 1, 2) are two horizontal circular plates of tinned brass. K is a vertical glass cylinder which is gripped between them. The two junctions are made fast with india-rubber bands.

Five vertical brass rods (of which two, $\beta_1\,\beta_2$, appear in the figure) hold the plates $b\,b'$ and cylinder K in position. The combination will in future be referred to as the "drum." $G_1\,G_2$ are two cylindrical tubes; the lower part of each is made of tinned brass, the upper of glass. They are destined to hold pure water and solution respectively. Outside and below the drum they terminate in a pair of three-way taps $t_1\,t_2$.

$P_1\,P_2$ are two pipettes, each passing through both lid and base of the drum. Each pipette can be connected to the corresponding G-tube in the manner shown in the plate (fig. 2). The upper extremities of the pipettes are placed in connection by a horizontal tube A.

B is a boiler, and the steam generated in it passes along A,

downwards, through the pipettes, and upwards through the G-tubes. It then spreads into the drum (thus jacketing the G-tubes), and finally passes through the waste W into the condenser C. This circulation is indicated by means of the arrows in the diagram (fig. 2). The condenser leads to a Woulfe's bottle V in which the pressure may be adjusted to any value.

A mercurial thermometer, T_0, passes through the lid b, and its bulb is near the centre of the drum. Its stem is jacketed by a tube, connected through a tap t_4, with the steam supply in A. If this tap is left *wide* open the steam from the generator flows almost entirely through it into the jacket tube and thus into the drum, avoiding the alternative circuit through the G-tubes. By regulating the tap t_4 we can therefore adjust the flow through the G-tubes with considerable nicety.

T_1 T_2 are two platinum thermometers, and by means of them the difference of the temperatures of pure water and solution, boiling under similar conditions, may be ascertained.

If it is desired to add or remove liquid to or from the G-tubes, the three-way taps t_1 t_2 are set so as to connect them with the reservoirs, n n, instead of with the pipettes P. When the operation is being carried on under ordinary pressure, the contents of the G-tubes will descend into the reservoir; on the other hand, by forcing air into the reservoir, any liquid therein will ascend into the G-tube. At reduced pressure the descent takes place as before, and in order to make liquid ascend it is no longer necessary to blow, but merely to open the three-way tap t_3 to the atmosphere. On doing this the reservoir r is placed out of connection with the low pressure in the Woulfe's bottle V, and into connection with the atmospheric pressure, and any liquid in the reservoir at once ascends.

Improvements.—When the apparatus came to be used, some improvements were made as the result of experience. It was found, for instance, that the drum was not able completely to prevent the condensation in the G-tubes, so that the depth of liquid in them tended to increase. As such changes of depth were found to be a source of error, at any rate when they proceeded unequally in the two G-tubes, it was necessary either to equalise or prevent them. Though this could easily be done by running off liquid from time to time through the three-way taps t_1 t_2, such a proceeding was for many reasons very undesirable. Moreover, the attention of the observer was quite sufficiently occupied at the bridge and galvanometer (§ 4).

In one series of experiments, therefore, a very small symmetrically shaped double ring-burner was placed with one half under each G-tube, and condensation was thus exactly balanced. As this burner *was not used in* the experiments already published (*loc. cit.*) it is

not necessary to consider it further, except to say that the numbers
in that series are in good accord with those in a series obtained by
the help of the ring-burner.

In the first-named series no attempt was made to prevent the
slight condensation. The burners were dispensed with and the
equalisation of depths or " levelling " was effected by an automatic
device to which special attention must be drawn.

(PLATE 2.)
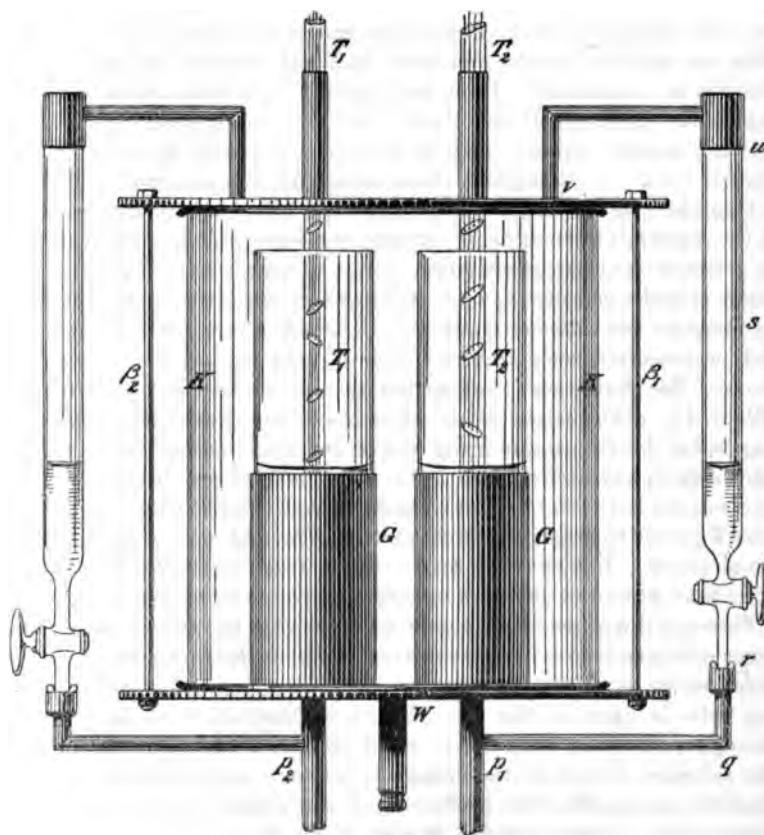
The two metal tubes by which the steam ascended into the G-tubes
were perforated at p_1 p_2 (Plate 2). From each a narrow metal
tube was carried horizontally as far as q, when it bends up-
wards as far as r. At this point it gives place to a burette s. The
top of the burette fits into a metal tube at u which bends round,
first horizontally then vertically, and finally passes through the lid

of the drum at *v*. If the three-way taps $t_1 t_2$ (Plate 1, not seen
Plate 2) are closed, any liquid in the G-tubes "finds its own level"
on the burettes, and then by means of the same three-way taps the
level in question may be adjusted to any desired mark on the burette.

If a current of steam from the boiler is now caused to pass
through the G-tubes, the liquid is dragged out of the burette into
the G-tubes.* The current of steam used in practice was never
sufficiently rapid to empty the burettes. Suppose, now, that conden-
sation proceeds more rapidly in one G-tube than in the other.
Owing to the increased hydrostatic pressures, the velocity of steam
on that side automatically diminishes, and in consequence some liquid
slips out of the G-tube and back into the burette, till the former
velocity is reproduced. It is not claimed that this device *entirely*
equalized the levels of the liquids, but it certainly tended in a very
marked way to do so. This is best demonstrated by closing the
burette taps. On doing this the readings become unsteady at once.

One more appliance has to be described affecting the adjustment
of the pressure in the drum. It was necessary that any fluctuations
in pressure (not necessarily the result of leaks) should at once be
detected and smoothed down. Neither mercurial thermometers nor
manometers are sufficiently rapid in their indications for this purpose,
and manometers consisting entirely of light liquids are very cum-
brous. The following combination proved to be what was wanted
(Plate 1). The bottle M is more than half full of mercury, the
remainder being occupied by water coloured red by fuchsin. A
rubber cork, having two holes, fills the mouth of the bottle, leaving
no air-space between itself and the surface of the red water. The tall
tube T passes through one hole of the cork and below the level of
the mercury. T is connected by a rubber tube to the Woulfe's bottle
V, so that when the latter is exhausted the mercury rises in T.

Through the other hole in the cork passes a capillary tube whose
lower end terminates in the coloured liquid without reaching down
so far as the surface of the mercury. The upper end of this capil-
lary tube is open to the air. About midway up it communicates
through a side tube with the funnel F, which is also open to the air.
The coloured liquid in the bottle M extends up the capillary tube
and half way up F. The usefulness of the apparatus turns on the
fact that if t_7 is open any fall in pressure in the Woulfe's bottle will
show itself mainly by a small rise of the mercury surface in T, but if
t_7 is closed, by a fall, about tenfold greater, of the light liquid in the
capillary. This is due to the fact that the cross-section of T is great
compared to that of the capillary. The apparatus can therefore be

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* Compare the action of Giffard's injector. It should be added that, when in
use, the whole system, *p, q, r, s,* was packed with canvas, except near the meniscus
in *s*.

made of low sensitiveness till the pressure is within a little of what is desired, and then by closing the tap t_7 its sensitiveness can be increased tenfold. Thus the observer, with his eye on the scale of the capillary and hand on the tap t_6, can make a momentary connection either with the exhaust pump or the external air, and so regulate the pressure with great precision.

§ 4. *The Electrical Instruments.*

So much for the mechanical portion of the apparatus : it remains to describe the electrical part.

The outfit consists of two Callendar-Griffiths platinum thermometers, a battery acting through a resistance of 50 ohms, a pair of "equal arm" coils, a galvanometer, and a bridge whose 80 cm. of wire balance the difference of resistance of the two thermometers, thus indicating the difference of their temperatures.

The method of compensating and connecting these thermometers is fully described by Griffiths ;* the bridge, however, will require a little comment. The wire was ordered to be 0·5 ohm to the metre, to which value it approached very nearly. It was furnished with the same platinum-silver wire as the standard instruments recently supplied to Kew Observatory. The scale unit was approximately 2 cm. The bridge itself was furnished with Professor Callendar's device for obviating the greater part of the thermo-electric effects by connecting the galvanometer to a wire running parallel to the bridge wire, and in all respects similar to it. These effects are further removed by a Griffiths's thermo-electric key.

All contacts were so designed as to fall within a very narrow compass, where they can if necessary be enclosed in a box to prevent injury. The equal arm coil was made of two pieces of wire having (when wound) precisely the same resistance and temperature coefficient. They are intertwined in a core of paraffin so as to be at equal temperatures. The actual temperature is then a matter of indifference.

The battery employed was a single dry cell. The galvanometer, after repeated attempts, was made very sensitive. The manner of reading its deflection differed somewhat from the ordinary one. The method is similar to that of Poggendorff, except that the scale is in the eye-piece of the telescope, which is focussed (by reflection from the galvanometer mirror) on a signal. Instead, therefore, of observing the movements of a scale relative to a fixed line in the focal plane of the telescope, one observes the movements of a line relative to a fixed scale in the focus of the telescope. In this there is nothing new. But no one, so far as can be ascertained, has

* 'Phil. Mag.,' January, 1895.

inquired what is the best relation of the distances between the observer and galvanometer on the one hand, and the signal and gal-vanometer on the other. Usually they are made equal, but a simple calculation shows that for a given angular displacement of the mirror the reading may be increased by bringing the observer near to the galvanometer and removing the signal to as great a distance as possible. Using this disposition of the galvanometer, very minute deflections of the mirror were plainly visible.

§ 5. *The employment of the Apparatus to measure the difference of the Boiling Points of Pure Water and Solution.*

The mode of employing the apparatus varies somewhat, according as it is required to work with weak or strong solutions, and at high or low pressures. The procedure with the weak solutions at 760 mm.* is as follows :—Steam from the generator is caused to circulate through the empty G-tubes and drum, and a position is found on the bridge, such that on making contact the galvanometer is undeflected. A reading of the bridge scale is taken and entered as "hypsometer null point." Water is now added to each G-tube, and caused to boil by the passage of steam. A similar null point is found and entered as "water null point." If the burettes (Plate 2) are properly adjusted the two null points are identical. In order now to measure the change in the boiling point due to the presence of a small quantity of salt, some water is removed from one G-tube, and replaced by a few c.c. of stock salt solution. The bridge is again balanced. It was found desirable that this balance should be obtained at the fifteenth minute of ebullition, and therefore at this moment the bridge must be correctly set, the levels duly adjusted, and the galvanometer observed to be undeflected. This is a work of considerable difficulty, but it may be accurately performed if all one's movements are regu-lated by the indications of a clock, so that each takes place at its appointed time. The observer ascertains, for instance, in a number of preliminary experiments, what gain takes place in the reading of the burettes during fifteen minutes, and in an actual experiment the levels are previously so adjusted that they shall be as nearly as possible correct at the end of such a period. The fifteen minutes may now be allotted to observations at the bridge and galvanometer, and then one may be sure of a close balance at the last minute. The levels of the burettes are finally recorded, and a sample of solution is withdrawn from the G-tubes for analysis. As a result of the pre-cautions described, the burette reading was almost invariably found to be within $1\frac{1}{2}$ mm. of the desired height. In any case a correction (L) can always be made at the rate of $0\cdot001°$ per $1\frac{1}{2}$ mm. inequality

* 'Roy. Soc. Proc.,' vol. 61, pp. 285—287.

in level. Another (G) is made, proportional to the swing of the galvanometer, if the observer has failed to procure an exact balance at the fifteenth minute.

§ 6. *Conclusion.*

This paper professes to be no more than a description of the method employed in obtaining the results.* I must therefore postpone till a later occasion all discussion of the precautions employed and of the accuracy obtainable. In conclusion, I wish to acknowledge my indebtedness to Mr. Griffiths for much invaluable assistance, and to Professor Thomson for permission to work in the Cavendish Laboratory.

February 17, 1898.

SIR JOHN EVANS, K.C.B., D.C.L., Treasurer and Vice-President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. " On the Connection between the Electrical Properties and the Chemical Composition of different kinds of Glass." By Professor ANDREW GRAY, LL.D., F.R.S., and Professor J. J. DOBBIE, M.A., D.Sc.

II. " On the Magnetic Deformation of Nickel." By E. TAYLOR JONES, D.Sc. Communicated by Professor ANDREW GRAY, F.R.S.

III. " Upon the Structure and Development of the Enamel of Elasmobranch Fishes." By CHARLES S. TOMES, M.A., F.R.S.

IV. " On artificial temporary Colour-blindness, with an Examination of the Colour Sensations of 109 Persons." By GEORGE J. BURCH, M.A. Communicated by Professor GOTCH, F.R.S.

V. " Contributions to the Mathematical Theory of Evolution. On the Inheritance of the Cephalic Index." By CICELY FAWCETT and KARL PEARSON, F.R.S.

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* ' Roy. Soc. Proc.,' vol. 61, pp. 285—287.

"Mathematical Contributions to the Theory ot Evolution. On the Law of Ancestral Heredity." By KARL PEARSON, M.A., F.R.S., University College, London. Received January 12,—Read January 27, 1898.

(A New Year's Greeting to Francis Galton, January 1, 1898.)

(1) *Introductory.*—In Mr. Galton's ' Natural Inheritance' we find a theory of regression based upon the "mid-parent." This formed the starting point of my own theory of biparental inheritance.[*] At the time Mr. Galton published his theory I venture to think that he had not clearly in view some of the laws of multiple correlation with which we are now more familiar. This certainly was my own condition when writing my memoir on heredity in 1895, and although in that memoir I pretty fully developed the theory of multiple correlation as applied to heredity, it had not then become such a familiar tool as two years' pretty constant occupation with it has since made it. Accordingly I misinterpreted a second principle of heredity propounded by Mr. Galton, and reached the paradoxical conclusion[†] that "a knowledge of the ancestry beyond the parents in no way alters our judgment as to the size of organ or degree of characteristic probable in the offspring." I assumed Mr. Galton to mean[‡] that the coefficients of correlation between offspring and parent, grandparent, great-grandparent, &c., were to be taken r, r^2, r^3, &c. The conclusions I drew from this result were, had the result been true, perfectly sound. The recent publication of Mr. Galton's paper on Basset hounds has led me back to the subject, because that paper contains facts in obvious contradiction with the principle above cited from my memoir of 1895. At first, I must confess, I was inclined to lay less stress on Mr. Galton's general law than it deserved, and attributed our divergence to the admitted roughness of colour data. After some correspondence with Mr. Galton and an endeavour on my part to represent his views in my own language, 1 have come to the conclusion that what I shall in future term *Galton's Law of Ancestral Heredity*, if properly interpreted, reconciles the discrepancies in ' Natural Inheritance' and between it and my memoir of 1895. It indeed enables us to predict *à priori* the values of all the correlation coefficients of heredity, and forms, I venture to think, the fundamental principle of heredity from which all the numerical data of inheritance can in future be deduced, at any rate, to a first approximation.

[*] ' Phil. Trans.,' A, vol. 187, pp. 253—318.

[†] *Ibid.*, p. 306.

[‡] I still think that this is the meaning to be extracted from pp. 132-5 of ' Natural Inheritance.'

The confidence I put in the truth of the law is not measured by Mr. Galton's researches on stature or on colour in Basset hounds, however strong evidence these may provide, but rather on the fact that the theory gives *à priori* the correlation between parents and offspring, and that this correlation is practically identical with the value I have myself determined from these and other observations.

With reservations as to how "mid-parent" shall be defined, I would state the law of ancestral heredity as follows:—

If k_s be the deviation of the sth mid-parent* from the mean of the sth ancestral generation, and k_0 be the probable deviation from the mean of the offspring of any individual, σ_s the standard deviation of the sth mid-parental generation, σ_0 of the generation of the offspring, then

$$k_0 = \tfrac{1}{2}\frac{\sigma_0}{\sigma_1}k_1 + \tfrac{1}{4}\frac{\sigma_0}{\sigma_2}k_2 + \tfrac{1}{8}\frac{\sigma_0}{\sigma_3}k_3 + \tfrac{1}{16}\frac{\sigma_0}{\sigma_4}k_4 + \ldots \; .$$

This is the somewhat generalised form of the law, which Mr. Galton sums up as "each parent contributes on an average one-quarter, or $(0\cdot5)^2$, each grandparent one-sixteenth, or $(0\cdot5)^4$, and so on, and that generally the occupier of each ancestral place in the nth degree, whatever be the value of n, contributes $(0\cdot5)^{2n}$ of the heritage."†

The generalised form above allows for a secular modification of the means and variabilities of the successive generations.

(2) Let r_1, r_2, r_3, r_4, ..., be the coefficients of correlation between offspring and parent, grandparent, great-grandparent, &c., respectively. Then, if correlation remains constant during the successive generations, $r_{n \sim s}$ would be the correlation between the parent of the nth generation and the parent of the sth generation if they be in the direct line of ascent, for one of these is the $(n \sim s)$th parent of the other. It must be remembered that if r_q be the correlation between an individual qth parent and his offspring, r_q may theoretically have a great variety of values according to the proportion and order of the sexes in the line of descent. If all these r_q's be unequal, then r_q shall be taken to represent their mean value. It will be necessary for our investigations to find the correlation between the nth and sth mid-parents in terms of these r's, which give the correlation between individuals. Let ρ_{ns} be the correlation between the nth and sth mid-parents. Let $_qh_s$ be the deviation of any organ of the qth male sth parent from the mean of that organ for the sth generation of male parents, $_qh'_s$ that of his female mate; let m be any constant not yet

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* A father is a first parent, a grandfather a second parent, a great-grandfather a third parent, and so on, in the notation here adopted. The mid sth parent or the sth mid-parent is derived from all 2^s individual sth parents.

† 'Roy. Soc. Proc.,' vol. 61, p. 402.

determined. Then there will be 2^{s-1} male and 2^{s-1} female sth parents, and the "mid-parent" will be defined to be an individual having a deviation from the mean of the sth parental generation

$$= \frac{{}_1h_s + {}_2h_s + {}_3h_s + \ldots + m({}_1h'_s + {}_2h'_s + {}_3h'_s + \ldots)}{2^s} .$$

This is a somewhat more general definition than Mr. Galton's.

Since $S(h) = 0$, $S(h') = 0$, when the summation extends over all individuals of the sth generation who are parents, it follows that the mean deviation of all possible sth mid-parents is zero.[*]

Next let us find the standard deviation Σ_s of the sth mid-parents. If their number for the population be N, then

$$\mathrm{N} \times \Sigma_s^2 = \frac{1}{2^{2s}} \mathrm{S} \{{}_1h_s + {}_2h_s + {}_3h_s + \ldots + m({}_1h'_s + {}_2h'_s + {}_3h'_s + \ldots)\}^2.$$

Now, if there be assortative mating, $S({}_qh_s . {}_qh'_s)$ will equal $\mathrm{N}\sigma_s\sigma'_s e_s$, where e_s is the correlation coefficient for assortative mating in the sth generation and σ_s, σ'_s the standard deviations of male and female mates in that generation. Further, $S({}_qh_s . {}_{q'}h_s)$ and $S({}_qh'_s . {}_{q'}h'_s)$ will not be absolutely zero, because assortative mating would mean a class mating into a like class leading to a correlation between relations in law; but these sums would be of the order e_s^2, and since, at any rate for man, e_s appears to be very small, we may neglect them to a first approximation.[†] Hence

$$\mathrm{N} \times \Sigma_s^2 = \frac{1}{2^{2s}} \{2^{s-1} \mathrm{S}({}_qh_s)^2 + m^2 2^{s-1} \mathrm{S}({}_qh'_s)^2 + 2m 2^{s-1} \mathrm{S}({}_qh_s . {}_qh'_s)\}, .$$

or $\qquad \Sigma_s^2 = \frac{1}{2^{s+1}}(\sigma_s^2 + m^2\sigma'^2_s + 2m\sigma_s\sigma'_s e_s)$ .... (i).

Now let us take m, which is at our choice, equal to σ_s/σ'_s, then

$$\Sigma_s = \frac{1}{2^{\frac{1}{2}s}} \sigma_s (1 + e_s)^{\frac{1}{2}} \ldots\ldots\ldots\ldots\ldots (ii).$$

In the next place let us find the coefficient of correlation between two mid-parents, say those of the nth and sth generations. We have

$$\mathrm{N} \times \Sigma_s \Sigma_n \rho_{ns} = \frac{1}{2^s 2^n} \mathrm{S}[\{{}_1h_s + {}_2h_s + {}_3h_s + \ldots + m({}_1h'_s + {}_2h'_s + {}_3h'_s + \ldots)\}$$
$$\times \{{}_1h_n + {}_2h_n + {}_3h_n + \ldots + m({}_1h'_n + {}_2h'_n + {}_3h'_n + \ldots)\}].$$

[*] Reproductive selection, which would weight particular parents, is neglected, or, if not, the h's must be measured from the weighted means of all sth parents.

[†] Here, as later, I exclude the effects of in-and-in breeding; this case requires special treatment. I hope shortly to publish fuller data for sexual selection in man, based upon a wider system of measurements than are dealt with in my memoir of 1895.

Now, if n be $>s$, any h_s will only (neglecting terms of order e^2) be correlated with its own particular $(n-s)$th parents, male and female, and there will be $\frac{1}{2}(2^{s-s})$ such male parents and $\frac{1}{2}(2^{s-s})$ such female parents. Hence

$$N \times \Sigma_s \Sigma_n \rho_{ns} = \frac{N}{2^{s+n}} \{ \sigma_s \sigma_n \times 2^{s-1} S_1(r_{sn}) + m \sigma'_s \sigma_n 2^{s-1} S_2(r_{sn})$$
$$+ m \sigma_s \sigma'_n 2^{s-1} S_3(r_{sn}) + m^2 \sigma'_s \sigma'_n 2^{s-1} S_4(r_{sn}) \},$$

$$\rho_{ns} = \frac{N \sigma_s \sigma_n}{2^s \Sigma_s \Sigma_n} \left\{ \frac{S_1(r_{sn}) + m \frac{\sigma'_s}{\sigma_s} S_2(r_{sn}) + m \frac{\sigma'_n}{\sigma_n} S_3(r_{sn}) + m^2 \frac{\sigma'_s \sigma'_n}{\sigma_s \sigma_n} S_4(r_{sn})}{2^{n-s+1}} \right\}$$

$$\cdots \cdots \cdots \text{(iii)}.$$

Here $S_1(r_{sn})$ is the sum of all r_{sn} which begin and end with male in the descent, $S_2(r_{sn})$, of those which begin with female and end with male, $S_3(r_{sn})$, of those which begin with a male and end with a female, and $S_4(r_{sn})$ of those which begin and end with a female. Now, as before, put $m = \sigma_s/\sigma'_s = \sigma_n/\sigma'_n$. We thus have, supposing the variability of each generation to be constant,

$$\rho_{sn} = \frac{2^{\frac{1}{2}(s-s)}}{1+e_s} \frac{S_1(r_{sn}) + S_2(r_{sn}) + S_3(r_{sn}) + S_4(r_{sn})}{2^{n-s+1}}$$

$$= \frac{2^{\frac{1}{2}(n-s)}}{1+e_s} r_{sn}, \quad \cdots \cdots \cdots \cdots \text{(iv)},$$

where r_{sn} now stands for the mean value of all the correlation co-efficients of an individual and its individual $(n-s)$th parents. It may be written $r_{n \sim s}$, as it depends only on the *difference* of the generations. Hence supposing sexual selection to remain constant, if it exists, for all generations, we see that ρ_{ns} depends only on the difference of generations, and may be written ρ_{n-s}, or:

$$\rho_{n-s} = 2^{\frac{1}{2}(n-s)} r_{n-s}/(1+e).^*$$

Now if there be no selective breeding, e appears, at any rate for man, to be small. Hence we have the important proposition:

> The correlation between two mid-parents, p generations apart, is equal to the product of $2^{\frac{1}{2}p}$ and the mean of the coefficients of correlation between an individual and its individual pth parents, when they are taken for all possible combinations of sex.

When no allowance is made for reproductive selection, it has been shown by Miss Alice Lee and myself that the four possible r's

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* The importance of this result is that it reduces the $\frac{n(n-1)}{1 \cdot 2}$ correlation coefficients between n mid-parents of different orders to n coefficients only.

for first parent and offspring are very nearly equal;[*] assuming the equality of all possible r_s's for the sth parent and offspring, and neglecting e we have

$$\left.\begin{array}{l} \Sigma_s = \sigma_s/2^{\frac{1}{2}s} \\[2mm] \rho_p = 2^{\frac{1}{2}p} r_p \end{array}\right\} \quad \dots\dots\dots\dots\dots \quad \text{(v)}.$$

or, we conclude that very approximately: *The standard deviation of the mid-sth parent may be obtained from the standard deviation of individual sth parents by dividing by $2^{\frac{1}{2}s}$, and the correlation between mid-sth parents and mid $(s + p)$th parents may be obtained by multiplying the correlation between an individual and any sth parent by $2^{\frac{1}{2}p}$.*

Thus the variability of the sth mid-parent rapidly decreases as we increase s, *i.e.*, as we get back in ancestry the mid-parent comes more and more nearly to represent in all cases the mean of the general population. Whether the correlation tends to decrease or increase will depend on the relative rates of change of $2^{\frac{1}{2}p}$ and r_p.

Since ρ_p must always be less than 1, we obtain at once the interesting limit that the correlation of an individual and a pth parent is always less than $(0.5)^{\frac{1}{2}p}$.

For example the correlation between :

Offspring and parent   must be less than 0·71
  ,,   and grandparents   ,,   ,,   0·5
  ,,   and great-grandparents   ,,   ,,   0·36
  ,,   and great-great-grandparents ,,   ,,   0·25

Their actual values as deduced from Mr. Galton's law are much smaller, as we shall see later.

(3) The reader will remark that in order to get these results in a simple form we have multiplied the female deviations from the mean by a constant factor m, which has afterwards been taken equal to the ratio of male to female variability. The reason for this was two-fold. In the first place σ is certainly not equal to σ', and, consequently, $m = 1$ would not have given

$$\Sigma_s = \frac{1}{2^{\frac{1}{2}s}}\sigma_s, \text{ but} = \frac{1}{2^{\frac{1}{2}(s+1)}} \sqrt{\sigma_s^2 + \sigma_s'^2},$$

a more complex form. In the next place we note the fairly close equality of r', r'', r''', r'''', when we neglect reproductive selection ; hence $m = \sigma_s/\sigma_s'$ is the only value which appreciably reduces formula (iii) as well as formula (i). I therefore define a mid-parent to be one in which the deviations of the females are reduced to the male standard by first multiplying them by the ratio of male to

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[*] ' Roy. Soc. Proc.,' vol. 60, p. 278.

female variability. This does not *theoretically* agree with Mr. Galton's definition, for he reduces the female to the male standard by multiplying them by the sexual ratio, or the ratio of the male to the female mean for the organ under consideration. In order, therefore, that my factor of reduction should agree with Mr. Galton's, it is needful that the ratio of the standard deviations should be equal to the ratio of the means, or, in other words, that the coefficient of variation should be the same for the two sexes. Now for the stature of men and women, I find for 1000 cases of each sex the coefficients 4·07 and 4·03 respectively, or the coefficient of variation is sensibly equal for both sexes.* Mr. Galton found from his anthropometric laboratory returns for somewhat fewer numbers, and probably for a lower social class, values of 3·75 and 3·79, again sensibly equal.† Hence the mid-parent, whether defined in my manner or in Mr. Galton's, would have a sensibly equal value in the case of stature, which is the one Mr. Galton dealt with in his 'Natural Inheritance.' The coefficient of variation is, however, not the same for both sexes in the case of all organs,‡ hence for the purpose of simplifying the formulæ, I am inclined to think my modification of Mr. Galton's original definition will prove of service.

(4) I shall now proceed to determine by the law of ancestral heredity the correlation between an individual and any *s*th parent from a knowledge of the regression between the individual and his mid-*s*th parent.

By the principles of multiple-correlation if x_0, x_1, x_2, ... x_n be $n+1$ organs, with standard deviations σ_0, σ_1, σ_2 .... σ_n, and correlations r_{01}, r_{02}, r_{03} .... r_{12}, r_{13} .... $r_{n-1, n}$, then the frequency surface is given by

$$z = \text{const} \times e^{-\frac{1}{2R}\left\{ R_{00}\left(\frac{x_0}{\sigma_0}\right)^2 + R_{11}\left(\frac{x_1}{\sigma_1}\right)^2 + ... + 2R_{01}\left(\frac{x_0 x_1}{\sigma_1 \sigma_2}\right) + ... \right\}},$$

where
$$R = \begin{vmatrix} 1, & r_{01}, & r_{02}, & r_{03} \cdots \cdots r_{0n} \\ r_{01}, & 1, & r_{12}, & r_{13}, \cdots \cdots r_{1n} \\ \cdots & \cdots & \cdots & \cdots \cdots \cdots \cdots \\ \cdots & \cdots & \cdots & \cdots \cdots \cdots \cdots \\ r_{0n}, & r_{1n}, & r_{2n} & \cdots \cdots \cdots r_{nn} \end{vmatrix},$$

and R_{pq} is the minor of the constituent of *p*th row and *q*th column.

* See 'The Chances of Death,' vol. 1, "Variation in Man and Woman," p. 294.
† *Ibid.*, p. 311. Mr. Galton's family record data gave 1·032 and 1·005 for the ratio of the coefficient of variation of sons to daughters and of fathers to mothers respectively. See 'Phil. Trans.,' A, vol. 187, p. 271.
‡ Many cases are given in the paper on "Variation in Man and Woman," cited above.

Putting $x_1, x_2, \ldots x_n$ constants, say $k_1, k_2, \ldots k_n$, we have for the mean value $\bar{x}_0$ of the corresponding array of x's,

$$\bar{x}_0 = -\left(\frac{R_{01}\sigma_0}{R_{00}\sigma_1}k_1 + \frac{R_{02}\sigma_0}{R_{00}\sigma_2}k_2 + \frac{R_{03}\sigma_0}{R_{00}\sigma_3}k_3 + \ldots + \frac{R_{0n}\sigma_0}{R_{00}\sigma_n}\right)k_n \ldots \quad \text{(vi)}.$$

The standard deviation of x_0 for this array is

$$\Sigma_0 = \sigma_0\sqrt{R/R_{00}} \quad \ldots\ldots\ldots\ldots\ldots \quad \text{(vii)}.$$

These results (vi) and (vii) are the regression formulæ.[*]

Now let $x_1, x_2 \ldots x_n$ be the mid-parental values of the 1st, 2nd, 3rd, ... nth order, and $\bar{x}_0 = k_0$ the mean value of the organ in the offspring.

Then the value of R is given by

$$R = \begin{vmatrix} 1, & \rho_1, & \rho_2, & \rho_3, & \rho_4 \ldots \rho_n \\ \rho_1, & 1, & \rho_1, & \rho_2, & \rho_3 \ldots \rho_{n-1} \\ \rho_2, & \rho_1, & 1, & \rho_1, & \rho_2 \ldots \rho_{n-2} \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ \rho_n, & \rho_{n-1} & \ldots\ldots\ldots\ldots 1 \end{vmatrix} \quad \ldots\ldots \quad \text{(viii)}.$$

and the regression formula is :

$$k_0 = -\left(\frac{R_{01}}{R_{00}}\frac{\sigma_0}{\Sigma_1}k_1 + \frac{R_{02}}{R_{00}}\frac{\sigma_0}{\Sigma_2}k_2 + \ldots + \frac{R_{0n}}{R_{00}}\frac{\sigma_0}{\Sigma_n}k_n\right),$$

if we stop at the nth mid-parent.

Comparing this result with the analytical statement of Mr. Galton's law of ancestral heredity given on p. 388, we see that we must have from (v) :

$$\left. \begin{array}{l} R_{01}/R_{00} = -\dfrac{1}{2}\dfrac{\Sigma_1}{\sigma_1} = -\dfrac{1}{2\sqrt{2}} \\[2mm] R_{02}/R_{00} = -\dfrac{1}{4}\dfrac{\Sigma_2}{\sigma_2} = -\left(\dfrac{1}{2\sqrt{2}}\right)^2 \\[2mm] \ldots\ldots\ldots\ldots\ldots\ldots\ldots \\[1mm] R_{0q}/R_{00} = -\dfrac{1}{2^q}\dfrac{\Sigma_q}{\sigma_q} = -\left(\dfrac{1}{2\sqrt{2}}\right)^q \end{array} \right\} \ldots\ldots\ldots \quad \text{(ix)}.$$

There will be n such equations, if we go to the mid-nth parent, and there are n quantities $\rho_1, \rho_2 \ldots \rho_n$ to find. Thus Mr. Galton's statement that the *partial* regression coefficients are $\frac{1}{2}, \frac{1}{4}, \frac{1}{8} \ldots$

[*] See 'Phil. Trans.,' A, vol. 187, p. 302.

gives us sufficient equations to find the coefficients of *total* correlation between the offspring and the successive mid-parents. Equations (v) will then enable us to find the coefficients of correlation between an individual and any individual ancestor. But these in their turn will suffice to determine all inheritance whether direct or *collateral* (see below). In short if Mr. Galton's law can be firmly established, *it is a complete solution, at any rate to a first approximation, of the whole problem of heredity.* It throws back the question of inheritance upon two constants, which can be once and for all determined; herein lies its fundamental importance. I must confess that this element of simplicity was at first my chief difficulty in accepting the law as laid down in the paper on Basset hounds, and I even yet have a certain hesitation, owing to an apparent difference in collateral heredity in different social classes, and also to the apparent numerical value of the inheritance of fertility in man.

(5) I shall next obtain a solution of equations (ix). Taking the minors of the $\overline{n+1}$ constituents of the first row, namely, R_{00}, R_{01}, R_{02}, ... R_{0n}, and multiplying them in succession by the constituents of the 2nd, 3rd .... $\overline{n+1}$th row of R, we have by the ordinary theory of determinants the system:

$$\rho_1 R_{00} + R_{01} + \rho_1 R_{02} + \rho_2 R_{03} + \ .... \ + \rho_{n-1} R_{0n} = 0.$$

$$\rho_2 R_{00} + \rho_1 R_{01} + R_{02} + \rho_1 R_{03} + \ .... \ + \rho_{n-2} R_{0n} = 0.$$

$$\bullet$$

$$\rho_q R_{00} + \rho_{q-1} R_{01} + \rho_{q-2} R_{02} + \rho_{q-3} R_{03} + \ .... \ + \rho_{n-q} R_{0n} = 0.$$

$$\bullet$$

$$\rho_n R_{00} + \rho_{n-1} R_{01} + \rho_{n-2} R_{02} + \rho_{n-3} R_{03} + \ .... \ + R_{0n} = 0.$$

Divide each of these equations by R_{00} and let us use instead of (ix) the somewhat more general system which will allow us to consider one or two limiting cases, and rather more generally than Mr. Galton has done "to tax the bequests of each generation," as he expresses it :*

$$\frac{R_{01}}{R_{00}} = -\gamma\beta, \quad \frac{R_{02}}{R_{00}} = -\gamma\beta^2, \quad \frac{R_{03}}{R_{00}} = -\gamma\beta^3, \quad \&c. \ .. \quad (x),$$

where γ and β are two constants; we then find:

* 'Natural Inheritance,' p. 135.

$$-\rho_1+\gamma\beta+\gamma\beta^2\rho_1+\gamma\beta^3\rho_2+\;\ldots.+\gamma\beta^n\rho_{n-1}=0.$$

$$-\rho_2+\gamma\beta\rho_1+\gamma\beta^2+\gamma\beta^3\rho_1+\;\ldots.+\gamma\beta^n\rho_{n-2}=0.$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$-\rho_q+\gamma\beta\rho_{q-1}+\gamma\beta^2\rho_{q-2}+\gamma\beta^3\rho_{q-3}+\;\ldots.+\gamma\beta^n\rho_{n-q}=0.$$

$$-\rho_{q+1}+\gamma\beta\rho_q+\gamma\beta^2\rho_{q-1}+\gamma\beta^3\rho_{q-2}+\;\ldots.+\gamma\beta^n\rho_{n-q-1}=0.$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$-\rho_n+\gamma\beta\rho_{n-1}+\gamma\beta^2\rho_{n-2}+\gamma\beta^3\rho_{n-3}+\;\ldots.+\gamma\beta^n=0.$$

Multiply the $\overline{q+1}$th equation by $1/\beta$, and subtract from the qth; we have

$$\frac{1}{\beta}\rho_{q+1}-\rho_q(1+\gamma)+\gamma\beta^n\rho_{n-q}=0\;\ldots\ldots\ldots\quad\text{(xi)}.$$

Assume $\rho_q=c\alpha^q$, hence:

$$\frac{\alpha}{\beta}-(1+\gamma)+\gamma\beta^n\alpha^{n-2q}=0.$$

But since α and β are both less than unity, the last term will be vanishingly small when n is indefinitely large, thus:

$$\alpha=\beta(1+\gamma)\;\ldots\ldots\ldots\ldots\ldots\quad\text{(xii)}.$$

Substituting $\rho_q=c\alpha^q$ in the first of the equations for the ρ's above, we have:

$$c\left(-\alpha+\gamma\beta\frac{\beta\alpha-\beta^n\alpha^n}{1-\beta\alpha}\right)=-\gamma\beta.$$

Or, taking as before $(\alpha\beta)^n=0$ for n very large:

$$c=\frac{1-\beta\alpha}{\alpha\left(\dfrac{1}{\gamma\beta}-\beta-\dfrac{\alpha}{\gamma}\right)},$$

$$c\alpha=\frac{1-\beta^2(1+\gamma)}{\dfrac{1}{\gamma\beta}-\beta\dfrac{(1+2\gamma)}{\gamma}},\ldots\ldots\ldots\ldots\quad\text{(xiii)}.$$

(xii) and (xiii) contain a complete solution of the fundamental equations for the ρ's given above, so long as we go only to a finite number of mid-parents, *i.e.*, q may be very large, but not comparable with $n=\alpha$.

(6) *Special Cases.*

(a) Put $\gamma=1$, $\beta=\frac{1}{2}$. It follows that $\alpha=1$, and $c=1$. Hence if

$$\frac{R_{0q}}{R_{00}}=-\frac{1}{2^q},$$

all the total mid-parental correlations would be perfect, and, there-
fore, any one mid-parent would suffice to fully determine any other
and the offspring. The individual parental correlations would then be

$$\frac{1}{\sqrt{2}}, \quad \frac{1}{2}, \quad \frac{1}{2\sqrt{2}}, \quad \frac{1}{4}, \quad \ldots \ldots$$

for parent, grandparent, great-grandparent, &c., with offspring.[*]

(*b*) More generally, suppose any values of γ and β which lead to
$c = 1$, then

$$c = 1 = \frac{1 - \beta^2(1+\gamma)}{\dfrac{\gamma+1}{\gamma}[1 - \beta^2(1+2\gamma)]},$$

whence we find $\beta(\gamma+1) = 1$, that is, $\alpha = 1$; or again, all mid-
parental correlations are perfect. Thus, as in case (i), the individual
parental correlations could be represented by

$$r, \quad r^2, \quad r^3, \quad \ldots \ldots .$$

These are the values I took in my memoir of 1895.[†] I took these
values then because they seemed to express Mr. Galton's method of
passing from individual parental to individual grand-parental total
regression.[‡] I had not perceived that there was any antinomy
between Mr. Galton's theory of regression and his law of ancestral
heredity. Had I done so I should certainly, at that date, have given
the preference to the former, and rejected his law of partial coeffi-
cients of regression in favour of the values, based on numerical
observation, of his total regression coefficients.

(*c*) Put $\gamma = 1$, $\beta = \dfrac{1}{2\sqrt{2}}$; this is Mr. Galton's form of the law.
We find at once

$$\alpha = \frac{1}{\sqrt{2}}, \qquad c = \frac{3}{5} = 0\cdot6.$$

Hence we have for the successive mid-parental correlations ρ_1, ρ_2,
ρ_3, &c.,

$$\frac{0\cdot6}{\sqrt{2}}, \quad 0\cdot3, \quad \frac{0\cdot3}{\sqrt{2}}, \quad \&c.$$

and for the individual *mean* parental correlations, r_1, r_2, r_3, &c.

$$0\cdot3, \quad 0\cdot15, \quad 0\cdot075, \quad \&c.$$

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[*] This is what, I think, must follow from any theory of the "continuity of the
germ plasma," and of its exact quantitative addition and bisection on sexual repro-
duction.

[†] 'Phil. Trans.,' A, vol. 187, pp. 303–5.

[‡] See 'Natural Inheritance,' p. 133. Mr. Galton puts $r = \frac{1}{3}$ for a parent,
$r^2 = \frac{1}{9}$ for a grandparent, and so on.

Here

$$r_1 = \tfrac{1}{4}(r_1' + r_1'' + r_1''' + r_1''''), \qquad r_2 = \tfrac{1}{3}S(r_2'), \quad \&c.$$

Further, for the regressions on the mid-parents (not partial but total), or $\rho_1\frac{\sigma_0}{\Sigma_1}$, $\rho_2\frac{\sigma_0}{\Sigma_2}$, $\rho_3\frac{\sigma_0}{\Sigma_3}$, &c., we have, on the assumption that all generations are equally variable,

$$0\cdot6, \quad 0\cdot6, \quad 0\cdot6, \quad \&c.$$

Or we may express the law of ancestral heredity in Mr. Galton's form in the following simple statement :—*The total regression of the progeny on the mid-parent of any generation is constant and equal to* 0·6.

Let us see how these results agree with observations. Mr. Galton[*] tells us that his first estimate of mid-parental regression was 3/5 = 0·6. This estimate exactly agrees with theory. He afterwards[†] changed the value to 2/3 = 0·67, which is less in agreement. My own calculations,[‡] on Mr. Galton's data, give $r_1' = 0\cdot3959$, $r_1'' = 0\cdot3603$, $r_1''' = 0\cdot2841$, $r_1'''' = 0\cdot3018$, or $r_1 = 0\cdot3355$ instead of 0·3. The probable error is, however, 0·026. If we do not weight fertility the parental correlation[§] = 0·41 ± 0·03, a value which is distinctly too high for Galton's law. It must be remembered, however, that our deductions from that law are based on equality of variation in each generation, and that this equality is by no means the fact. I hope shortly to get final values for parental heredity from my family measurements, which have now reached a total of nearly 1,100 families, and thus settle how far Galton's law needs to be modified. On the whole the confirmation obtained from stature data for the law of ancestral heredity is very striking;[||] I am inclined to think even more convincing than that obtainable from the Basset hounds, and this for a reason to be considered later. It suffices here to observe that we do not need to know the characters of parents, grand-parents, great grand-parents to test Mr. Galton's law; any single relationship, near or far, direct or collateral (see below), will bring its quota of evidence for or against the law.

It will be seen that the table (p. 397) differs in principle from Mr. Galton's on p. 133 of his 'Natural Inheritance.' In particular, supposing equal variability for all generations, the individual grand-parental regression is not the square of the parental regression, but the *half* of it. Mr. Galton's law of ancestral heredity contradicts

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[*] 'Natural Inheritance,' p. 97.

[†] *Ibid.*, p. 97.

[‡] 'Phil. Trans.,' A, vol. 187, p. 270.

[§] 'Roy. Soc. Proc.,' vol. 60, p. 279.

[||] Good evidence in its favour is also to be deduced from the inheritance of the cephalic index. See paper by Fawcett and Pearson, *infrà*, p. 413.

Table of Heredity according to Galton's Law.

Order.	Individual parent. Correlation and regression.	Mid-parent. Correlation.	Mid-parent. Regression.
1	0·3000	0·4243	0·6
2	0·1500	0·3000	0·6
3	0·0750	0·2121	0·6
4	0·0375	0·1500	0·6
5	0·01875	0·1061	0·6
6	0·009375	0·0750	0·6
qth	$0 \cdot 6 \left(\dfrac{1}{2}\right)^q$	$0 \cdot 6 \left(\dfrac{1}{\sqrt{2}}\right)^q$	0·6

Remarks.—The correlation of the individual first parent is to be taken as the mean of the four possible parental correlations due to differences of sex, if these are not sensibly equal, and a like rule holds for the individual *s*th parent. The individual parental regression is based on the assumption that the variability of offspring and parent are the same. In dealing with the mid-parent, female deviations must first be reduced to male by multiplying them by the ratio of male to female variability.

his views on regression, and it is the latter which, judging from both theory and observation, I now hold must be discarded.[*]

(7) Mr. Galton's law gives us the partial regression coefficients when *all* the mid-parents are known. It is desirable to deduce from the theory of multiple correlation the values of the partial regression coefficients when we take 1, 2, 3, 4,.... mid-parents only. When q mid-parents are taken let the partial regression coefficients be ϵ_{1q}, ϵ_{2q}, ϵ_{3q}, ϵ_{4q}, ... ϵ_{qq}; then again we have for the mean of the offspring k_0:

$$k_0 = \epsilon_{1q} \frac{\sigma_0}{\sigma_1} k_1 + \epsilon_{2q} \frac{\sigma_0}{\sigma_2} k_2 + \ \ldots + \epsilon_{qq} \frac{\sigma_0}{\sigma_q} k_q \ \ldots \ldots \quad \text{(xiv)},$$

where σ_0 is the standard deviation of the offspring and σ_p of the *p*th parental generation. Comparing this with the regression formula immediately under (viii) we have

$$\epsilon_{pq} = -\frac{R_{0p}}{R_{00}} \frac{\sigma_p}{\Sigma_p} = -2^{\frac{1}{2}p} \frac{R_{0p}}{R_{00}}, \text{ by (v).}$$

[*] I do not agree with the last column of Mr. Galton's table giving the variability of arrays. For single correlation the variability (standard deviation) of an array $= \sigma \sqrt{1 - r^2}$, where r is the correlation and *not* the regression. With equal variability of all generations, r in the case of the individual parent may be replaced by the regression. But the correlation is not equal to the regression in the case of mid-parents, because the variability of the mid-parent by (v) is increasingly less than that of the offspring.

Now make use of the general equations for the ρ's given just below equation (x), substituting for the R's in terms of the ϵ's, and remembering that we are to stop at $n = q$, that $\rho_p = ca^p$, and that $1/\sqrt{2} = a$. We have after some reductions the system :

$$1 = \frac{1}{c}\epsilon_{1q} + a^2\epsilon_{2q} + a^4\epsilon_{3q} + a^6\epsilon_{4q} + \dots + a^{2q-2}\epsilon_{qq},$$

$$1 = \epsilon_{1q} + \frac{1}{c}\epsilon_{2q} + a^2\epsilon_{3q} + a^4\epsilon_{4q} + \dots + a^{2q-4}\epsilon_{qq},$$

$$1 = \epsilon_{1q} + \epsilon_{2q} + \frac{1}{c}\epsilon_{3q} + a^2\epsilon_{4q} + \dots + a^{2q-6}\epsilon_{qq},$$

$$\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots$$

$$\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots$$

$$1 = \epsilon_{1q} + \epsilon_{2q} + \epsilon_{3q} + \dots + \frac{1}{c}\epsilon_{q-1\,q} + a^2\epsilon_{qq},$$

$$1 = \epsilon_{1q} + \epsilon_{2q} + \epsilon_{3q} + \dots + \epsilon_{q-1\,q} + \frac{1}{c}\epsilon_{qq}.$$

Subtracting the $(q-1)$th of these equations from the qth, the $(q-2)$th from the $(q-1)$th, &c., and introducing the values of $a^2 = \frac{1}{2}$ and of $c = 0\cdot6$, we find

$0\cdot4\,\epsilon_{q-1\,q} - 0\cdot7\,\epsilon_{qq} = 0.$

$0\cdot4\,\epsilon_{q-2\,q} - 0\cdot7\,\epsilon_{q-1\,q} - 0\cdot15\,\epsilon_{qq} = 0 \dots\dots\dots\dots\dots\dots$ (xv).

$0\cdot4\,\epsilon_{q-3\,q} - 0\cdot7\,\epsilon_{q-2\,q} - 0\cdot15\,\epsilon_{q-1\,q} - 0\cdot075\,\epsilon_{qq} = 0.$

$0\cdot4\,\epsilon_{q-4\,q} - 0\cdot7\,\epsilon_{q-3\,q} - 0\cdot15\,\epsilon_{q-2\,q} - 0\cdot075\,\epsilon_{q-1\,q} - 0\cdot375\,\epsilon_{qq} = 0.$

and so on, each new coefficient being now half the last. These equations give successively the ratios of $\epsilon_{q-1\,q}$, $\epsilon_{q-2\,q}$, $\epsilon_{q-3\,q}$, &c., to ϵ_{qq}. Hence the last of the previous set of equations will then give ϵ_{qq}. Thus the partial regression coefficients for any limited number of mid-parents can be found. This last equation also gives us

$$S(\epsilon) = 1 - \frac{1-c}{c}\epsilon_{qq} = 1 - \frac{2}{3}\epsilon_{qq},$$

a convenient formula for measuring how nearly the mean offspring of q mid-parents, all selected with a peculiar character, $k_1 = k_2 = k_3 = \dots = k_q = K$ has attained that character. For in this case

$$k_0 = S(\epsilon k) = K \times S(\epsilon),$$

and

$$k_0/K = 1 - \frac{2}{3}\epsilon_{qq} \dots\dots\dots\dots\dots (xvi).$$

Hence the more nearly $1-\frac{2}{3}\epsilon_{qq} = $ unity the more nearly the offspring has the full character of its selected parentage. I venture to call this expression the *stability of the stock*. It is a measure of the stock breeding true.

Lastly, to find the standard deviation of the array $= \sigma_0\sqrt{R/R_{00}}$ we have only to express R in terms of the minors of its first row, or,

$$R = R_{00} + \rho_1 R_{01} + \rho_2 R_{02} \ldots + \rho_q R_{0q},$$

$$R/R_{00} = 1-c\left(\epsilon_{1q}a^2 + \epsilon_{2q}a^4 + \epsilon_{3q}a^6 + \ldots + \epsilon_{qq}a^{2q}\right)$$

$$= 1-0\cdot3\left(\epsilon_{1q} + \frac{1}{2}\epsilon_{2q} + \frac{1}{4}\epsilon_{3q} + \ldots + \frac{1}{2^{q-1}}\epsilon_{qq}\right)\ldots\ldots \quad \text{(xvii)}.$$

In the limit when $q = \infty$

$$R/R_{00} = 1-0\cdot3\times\tfrac{1}{2}(1+\tfrac{1}{4}+\tfrac{1}{16}+\ldots) = 1-0\cdot2 = 0\cdot8,$$

and $$\sqrt{R/R_{00}} = 0\cdot8944.$$

The following table has been calculated from these formulæ:—

Table of Pedigree Stock according to Galton's Law.[*]

Number of genera-tions.	Ratio of variability of offspring to that of whole population.	Partial regression coefficients.						Stability. S (ϵ).
		ϵ_1.	ϵ_2.	ϵ_3.	ϵ_4	ϵ_5.	ϵ_6.	
1	0·9055	0·6	—	—	—	—	—	0·6000 (0·5)
2	0·8946	0·5122	0·2927	—	—	—	—	0·8049 (0·75)
3	0·8945	0·5015	0·2553	0·1459	—	—	—	0·9027 (0·875)
4	0·89445	0·5002	0·2507	0·1276	0·0729	—	—	0·9514 (0·9375)
5	0·8944	0·5000	0·2501	0·1253	0·0638	0·0365	—	0·9717 (0·9687)
6	0·8944	0·5000	0·2500	0·1250	0·0627	0·0319	0·0182	0·9879 (0·9844)
∞	0·8944	0·5000	0·2500	0·1250	0·0625	0·03125	0·015625	1

[*] To save possible labour, in case it should ever be needed to investigate the partial regression coefficients for more generations, I place here the ratios of the first six ϵ's :

$\epsilon_{q-1\,q}/\epsilon_{qq} = 1\cdot75$; $\epsilon_{q-2\,q}/\epsilon_{qq} = 3\cdot4375$; $\epsilon_{q-3\,q}/\epsilon_{qq} = 6\cdot859{,}375$.

$\epsilon_{q-5\,q}/\epsilon_{qq} = 13\cdot714{,}843$; $\epsilon_{q-6\,q}/\epsilon_{qq} = 27\cdot428{,}709$.

(8) I venture to think this table of considerable suggestiveness, and will now point out some of the conclusions that may be drawn from it.

(i) With a view of reducing the *absolute* variability of a species it is idle to select beyond the grandparents, and hardly profitable to select beyond parents. The ratio of the variability of pedigree stock to the general population decreases 10 per cent. on the selection of parents, and only 11 per cent. on the additional selection of grandparents. Beyond this no sensible change is made. We cannot then reduce variability beyond 11 per cent. by the creation of a pedigree stock, *i.e.*, by breeding from selected parents for 2, 3, 4, ... *n* generations. In some cases of course we appear to decrease variability—for example, if we *increase* the average size of an organ—for the *absolute* variability is then a smaller proportion of the actual size, and the relative variability, or coefficient of variation, may thus be steadily decreased. If Mr. Galton's law be true, then pedigree stock would retain only a slightly diminished capacity for variation about the new type. For example, the absolute variability of men of average height, 69·2 inches, being 2·6 inches, the absolute variability of men of 72 inches, obtained by selecting any number of 6-foot ancestors, would hardly fall short of 2·3 inches.[*]

(ii) Two different classes of pedigree stock exist. In the one we start with the general population, and select special characters for 1, 2, 3, ... *n* generations. In the other we know the pedigree for 1, 2, 3, ... *n* generations, but have no reason for supposing that before these generations the stock was absolutely identical with the general population.

In the former case we put for the mid-parents

$$k_1 = k_2 = k_3 = \ldots = k_n = K, \quad k_{n+1} = k_{n+2} = \ldots = k_\alpha = 0.$$

Hence the regression formula is

$$k_0 = \left(\tfrac{1}{2} + \tfrac{1}{4} + \tfrac{1}{8} + \ldots + \frac{1}{2^n} \right) K = \left(1 - \frac{1}{2^n} \right) K.$$

The values of k_0/K are tabulated in the last column of the table above in brackets. They give the ratio of character in offspring to character in ancestors, if ancestors of equal full character have been selected for *n* generations. We see that in six generations the offspring will have been raised to within 1·6 per cent. of the selected ancestral character.

In the latter case we must use the partial regression coefficients

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[*] The probability of an individual of selected stock differing widely from the type is of course much less than in the general population, because the stock is, *as a rule,* far less numerous.

ϵ_1, ϵ_2 .... of the table. For example, in the case of Mr. Galton's Basset hounds, 0·5015, 0·2553, and 0·1459 were the coefficients to be used, rather than 0·5, 0·25, and 0·125, when he proceeded to apply the law to three generations. These give the proper allowance for the ancestry beyond the pedigree. Thus the great-grandparents ought to have been given about a fifth more weight. If we proceed to six generations in pedigree stock of the latter type then the offspring will be within 1·2 per cent. of the selected ancestry, *i.e.*, their stability as given by the last column = 0·9879.

(iii) Now let us apply these results to the all-important problem of panmixia and degeneration. Suppose a selection made of a particular character for n generations, starting from the general population. Then the offspring in the $(n+1)$th generation will have $1 - \dfrac{1}{2^n}$ of the character on the average. Now, stopping selection, let us breed with a first generation of mid-parents with $1 - \dfrac{1}{2^n}$ of the character. The offspring will have:

$$\frac{1}{2}\left(1 - \frac{1}{2^n}\right) + \frac{1}{4} + \frac{1}{8} + \frac{1}{16} + \cdots + \frac{1}{2^{n+1}}$$

$$= \frac{1}{2}\left(1 - \frac{1}{2^n}\right) + \frac{1}{2}\left(1 - \frac{1}{2^n}\right) = 1 - \frac{1}{2^n} \text{ of the character.}$$

The $\overline{n+2}$th generation will have:

$$\frac{1}{2}\left(1 - \frac{1}{2^n}\right) + \frac{1}{4}\left(1 - \frac{1}{2^n}\right) + \frac{1}{8} + \frac{1}{16} + \cdots + \frac{1}{2^{2n+2}}$$

$$= \frac{1}{2}\left(1 - \frac{1}{2^n}\right) + \frac{1}{4}\left(1 - \frac{1}{2^n}\right) + \frac{1}{4}\left(1 - \frac{1}{2^n}\right) = 1 - \frac{1}{2^n}$$

of the character, and so on. The law is obvious; the offspring will always have the same amount of the character as had the generation after selection ceased. If we start with pedigree stock with *unknown* ancestry beyond the nth generation, we reach the same conclusion. Thus, after three generations the offspring will have 0·9027 of the selected parents' character. Now stop selection and the fourth generation will have:

$$0·9027\,\epsilon_1 + \epsilon_2 + \epsilon_3 + \epsilon_4$$

$$= 0·4515 + 0·2507 + 0·1276 + 0·0729 = 0·9027,$$

the fifth generation will have

$$0·9027\,(\epsilon_1 + \epsilon_2) + \epsilon_3 + \epsilon_4 + \epsilon_5 = 0·9027,$$

again, and so on. The general law is obvious.

Thus, on the basis of the law of ancestral heredity the case against panmixia is even stronger than it appeared in my memoir on heredity.[*] Assuming Mr. Galton's law of regression, I there showed that panmixia was possible with a stable focus of regression, but that the supporters of the consistent theory of panmixia must place that focus of regression, in order that degeneration should be continuous, in a position inconsistent with observed facts (p. 314). We now see that with the law of ancestral heredity even this is not possible, a race with six generations of selection will breed within 1·2 per cent. of truth ever afterwards, unless the focus of regression instead of being steady actually regredes. Of course there are many ways in which this law may be modified. For example, fertility may be a maximum with the average, say, of the unselected original population, and after a selection it may remain correlated, having the lesser values of the selected character more fertile than others.[†] Then, of course, the stock would degenerate with panmixia.[‡] This would, however, be reproductive selection, not panmixia in the ordinary significance, reversing natural selection. We are far too ignorant at present of the correlation of fertility with other characters to base any sweeping principle like that of degeneration by panmixia upon it. Our attitude at present can only be that there are no facts, and that there is no workable theory of heredity yet discovered which favours in any way degeneration by panmixia.

(9) *Taxation of Inheritance.*—If we assume Mr. Galton's law of ancestral heredity to be a limiting statement, we can at once from our general formulæ ascertain the influence of "taxing the inheritance" in any other than Mr. Galton's form. He has, in fact, taxed the inheritance (where by "inheritance" I understand deviation from the mean of the general population, not actual size of the character), 50 per cent. in each transmission. There may, however, be two types of taxation, a general taxation on the individual receipts and a special tax on each transmission—corresponding, so to speak, to a duty paid by an individual on coming into receipt of the entire ancestral property, and a stamp duty on each conveyance of an individual ancestor's contribution. The first is represented by the γ of our equation (x), and the second by the $\sqrt{2\beta}$.

Mr. Galton, in his memoir on Basset hounds, has stated certain conditions of the law of ancestral heredity, and he concludes (p. 403) that his conditions are only fulfilled by the series

$$\tfrac{1}{2} + (\tfrac{1}{2})^2 + (\tfrac{1}{3})^2 + \ldots$$

[*] 'Phil. Trans.,' A, vol. 187, p. 308 *et seq.*

[†] This is how I should at present account for the degeneration of pedigree wheat.

[‡] Some influence of this kind is possibly sensible in highly civilised communities. See "Reproductive Selection," in my 'Chances of Death,' vol. 1, pp. 98 *et seq.*

It seems to me that they are equally well fulfilled by the series

$$\gamma\beta' + \gamma\beta'^2 + \gamma\beta'^3 + \ldots,$$

provided the sum of this series is equal to unity,

or $\qquad \gamma\beta'/(1 - \gamma\beta') = 1$, that is $\gamma\beta' = \frac{1}{2}$.

But $\qquad \gamma\beta'^q = -\dfrac{R_{0q}}{R_{00}} \dfrac{\sigma_q}{\Sigma_q} = \gamma\beta^q \times (\sqrt{2})^q$ by (x),

or $\beta' = \sqrt{2}\beta$ of our previous notation.

Hence the conditions laid down are fulfilled by our general solution (x) provided

$$\gamma\beta = \frac{1}{2\sqrt{2}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{(xvii)}.$$

I do not assert that such a law is more probable than Mr. Galton's, or indeed as simple. But it throws back the theory of inheritance on at least one arbitrary constant γ, and therefore while covering Mr. Galton's law of ancestral heredity ($\gamma = 1$), allows a greater scope for variety of inheritance in different species.

It seems worth while to notice the changes that result in ancestral correlation when we put on a total " tax " γ. As a numerical illustration, take this tax at 10 per cent., then $\gamma = \frac{9}{10}$. We find

$$\beta = 0\cdot39284, \text{ and by (xii) and (xiii)}:$$

$$\alpha = 0\cdot74639. \quad c = 0\cdot58953.$$

From these values we can form a table exactly like that on p. 397. On examination of it, we see that the effect of a " general tax " is to increase sensibly all the correlations. In particular the more distant ancestry play a relatively greater part than they would do under Mr.

Table of Heredity. Tax 10 per cent.

Order.	Individual parent.	Mid-parent.	
	Correlation and regression.	Correlation.	Regression.
1	0·3111	0·4400	0·6223
2	0·1642	0·3284	0·6569
3	0·0867	0·2451	0·6933
4	0·0457	0·1830	0·7319
5	0·0241	0·1366	0·7725
6	0·0127	0·1019	0·8154
qth	0·5895$(0\cdot5278)^q$	0·5895$(0\cdot7464)^q$	0·5895$(1\cdot0556)^q$

Galton's unmodified law. Now the direct correlations as given by that law certainly appear somewhat small for both stature and cephalic index in man. Hence it is quite possible that when more extensive data are forthcoming, it will be found necessary to modify Mr. Galton's form and take γ less than unity. The above table will suffice to indicate the general direction of the correlation changes which result when γ is varied. One point should be noticed, the *total* regression on an individual mid-parent (note, not the *partial* regression) continually increases as we go further back, and will ultimately be greater than unity; in our case this will happen at the 10th generation. Now in such a generation an individual has 1024 10th great-grandparents, and, were they independent, the mean of these could hardly differ widely from the population mean. Hence the total regression coefficient being greater than unity is not so significant as it might at first sight seem. What it amounts to is this : that if we only knew of an individual that his mid-parent in a very distant generation had more of a character than the then population mean, and knew nothing about his other mid-parents, then the individual would probably have more of that character than the mid-parent. The apparent paradox arises from the very small variability of a distant mid-parent, and hence the extreme improbablility of a mid-parent differing very widely from the population mean. Of course with close in-and-in breeding the modification introduced by assortative mating could not be neglected, and our whole investigation would need modification.* Until, however, we have more measurements to deal with, it is idle to develop at length all the consequences which flow from the generalised form of Galton's law.

(10) *Collateral Heredity.*—There is another point on which the law of ancestral heredity gives us full information, namely, the correlation between brothers, cousins, and all other collateral relatives. In my memoir of 1895, I felt bound to reject Mr. Galton's regression coefficient for brothers, because its value seemed to me in contradiction with experience. I wrote (p. 285) :—

" There is not, I think, sufficient ground at present for forming any definite conclusion as to the manner in which lineal is related to collateral heredity. It does not seem to me necessary that the coefficient for the former should be half that for the latter, as supposed by Mr. Galton."

And again :—

* I hope to return to this point again. We have neglected e in equations (ii) and (iv). In endeavouring to follow back my own family to its fourth, and even sixth great-grandparents, I was surprised to find only one first and one second cousin marriage among the ascertainable ancestors. According, however, to O. Lorenz ('Lehrbuch der Genealogie,' s. 305), the present German Emperor has only 44 instead of 64 sixth parents, and 275 instead of 4096 twelfth parents !

" I doubt whether the correlation coefficients for collateral heredity—at any rate in the middle classes—can be greater than $0\cdot5$."

Strangely enough Mr. Galton's law of ancestral heredity which I then rejected, while accepting a part of what I now consider his erroneous theory of regression, gives just the link between linear and collateral heredity which I was then seeking !

Let x_1 and x_2 be the deviation of two brothers forming part of the array having

$$k_0 = \gamma\beta' k_1 + \gamma\beta'^2 k_2 + \gamma\beta'^3 k_3 + \ \ldots.$$

for its mean, x_1 and x_2 being measured from the general population mean. Let x_1 and x_2 differ from the mean of the array by x' and x'', then

$$x_1 x_2 = (k_0 + x')(k_0 + x'').$$

Now let us sum $x_1 x_2$ for all pairs of brothers in a given array, then since x' and x'' correspond to any pair of chance deviations within the array $S(x'x'') = 0$, $S(x') = S(x'') = 0$. Hence for the array $S(x_1 x_2) = S(k_0^2) = 2nk_0^2$,[*] where n is the number of pairs of brothers in the population with the series of mid-parents k_1, k_2, k_3, &c. Now write

$$k_0 = \gamma\beta \frac{\sigma_0}{\Sigma_1} k_1 + \gamma\beta^2 \frac{\sigma_0}{\Sigma_2} k_2 + \gamma\beta^3 \frac{\sigma_0}{\Sigma_3} k_3 + \ldots.$$

---

[*] The appearance of the 2 here requires notice. Let there be ν brothers to the array of any single series of mid-parents; then if s be the standard-deviation of the array, the distribution of brothers corresponding to a given k_0

$$= \frac{\nu}{\sqrt{2\pi s}} e^{-\frac{1}{2}(x'/s)^2}.$$

Hence the frequency of a brother between x' and $x' + \delta x'$ occurring with a brother between x'' and $x'' + \delta x''$

$$= \frac{\nu^2}{\sqrt{2\pi s^2}} e^{-\frac{1}{2}(x'/s)^2} dx' \, e^{-\frac{1}{2}(x''/s)^2} dx''.$$

If we take as limits x' and x'', both $= +\infty$ to $-\infty$, we shall clearly take each brother *twice* over with each other brother. Hence—

$$S(x_1 x_2) = \tfrac{1}{2} \int_{-\infty}^{+\infty}\int_{-\infty}^{+\infty} \frac{\nu^2}{2\pi s^2} (k_0 + x')(k_0 + x'') e^{-\frac{1}{2}\left\{(x'/s)^2 + (x''/s)^2\right\}} dx' \, dx'' = \tfrac{1}{2}\nu^2 k_0^2.$$

Now allow one pair of brothers to each system of mid-parents, and

$$S(x_1 x_2) = 2k_0^2$$

for one mid-parental system, or if there be n such mid-parental systems,

$$S(x_1 x_2) = 2nk_0^2.$$

Actually the same mid-parental system may be repeated many times, only in this case the possible correlation of fertility with the character under discussion must be guarded against.

and note that

$$S(nk_q{}^2) = N\Sigma_q{}^2, \quad S(nk_q k_{q'}) = N\Sigma_q \Sigma_{q'} \rho_{\pi q},$$

where σ_0 is the standard deviation of the offspring, N is the total number of pairs of brothers or mid-parents of each order, and Σq is as before the standard deviation of the group of qth mid-parents. Noticing that $\rho_{q' \sim q} = ca^{(q' \sim q)}$, we have if r be the correlation between brothers

$$N\sigma_0{}^2 r = S[S(x_1 x_2)] = 2S(nk_0{}^2)$$
$$= 2N\sigma_0{}^2 S(\gamma^2\beta^{2q} + 2\gamma^2\beta^{q+q'} ca^{(q'-q)}),$$

the sum now referring to all values of q and q' from 1 to ∞, q being unequal to q', and $q' \sim q$ taken positive.

Thus:
$$r = 2\gamma^2(\beta^2 + \beta^3 ca + \beta^4 ca^2 + \beta^5 ca^3 + \ \ldots$$
$$+ \beta^3 ca + \beta^4 + \beta^5 ca + \beta^6 ca^2 + \ \ldots$$
$$+ \beta^4 ca^2 + \beta^5 ca + \beta^6 + \beta^7 ca + \ \ldots$$
$$+ \beta^5 ca^3 + \beta^6 ca^2 + \beta^7 ca + \beta^8 + \ \ldots$$
$$+ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots)$$

Hence summing parallel to the diagonal:

$$r = 2\gamma^2 \left\{ \frac{\beta^2}{1-\beta^2} \left(1 + \frac{2ca\beta}{1-a\beta} \right) \right\} \ldots\ldots\ldots \text{(xviii).}$$

$$= \frac{2\gamma^2}{8\gamma^2 - 1 - 2\gamma} \text{ by (xii), (xiii), and (xvii).}$$

Let us evaluate this on Mr. Galton's law and on the hypothesis of a 10 per cent. tax.[*] On the first hypothesis $\beta = \dfrac{1}{2\sqrt{2}}$, $a = \dfrac{1}{\sqrt{2}}$, and $\gamma = 1$, hence $r = 0\cdot4$. On the second hypothesis (p. 403) $\beta = 0\cdot39284$, $a = 0\cdot74639$, and $\gamma = 0\cdot9$; hence $r = 0\cdot4402$.

We can also obtain less accurate values of fraternal correlation in other ways. Suppose two brothers to be considered as sons of one mid-parent k_1 only. In this case we must take $0\cdot6$ for the regression (see the table, p. 403), or

$$x_1 = 0\cdot6 k_1 + x', \qquad x_2 = 0\cdot6 k_1 + x'',$$

and as before :
$$S(x_1 x_2) = 2 \times (0\cdot6)^2 \times S(nk_1{}^2),$$
$$N\sigma_0{}^2 r = 2 \times 0\cdot36 \times N\Sigma_1{}^2,$$
$$r = 0\cdot36.$$

---

[*] [The above value for fraternal correlation shows that γ must be $> 0\cdot6076$; that a must be < 1, only gives $\gamma > 0\cdot5469$.]

If we suppose two individual parents with no assortative mating, we have

$$r = 2(r_1{}^2 + r_2{}^2) \dots\dots\dots\dots\dots\dots \quad \text{(xix)},$$

where r_1 and r_2 are the male and female parental correlations. With Galton's law $r_1 = r_2 = 0·3$, and r again $= 0·36$. Assuming the value $r_1 = r_2 = \frac{1}{3}$ adopted by Mr. Galton in his 'Natural Inheritance' (p. 133) for parental regression, the fraternal regression deduced from this ought to have been $\frac{4}{9} = 0·44$, and not $0·67$ as obtained by Mr. Galton.[*] The mean of the sister-sister, brother-brother, brother-sister correlations that I found in 1895,[†] duly weighted for the number of pairs in each case, is exactly $0·4000$. The value as it might have been *à priori* predicted from Galton's law $= 0·4000$, with a rise to $0·4402$, if we " tax " up to 10 per cent.

I conclude therefore that this law of ancestral heredity is at least to a first approximation in agreement as complete as could possibly be expected with the facts we as yet know as to collateral heredity. It confirms the view I took in 1895, that fraternal heredity cannot be taken greater than $0·5$. I think the high value (about $0·6$) obtained from Mr. Galton's " special data " must be explained by my suggested cause[‡] (*a*) *i.e.*, unconscious selection of approximately equal heights in brothers who join Volunteer regiments; for the explanation (*b*) is taken away if we accept Galton's law without a modified γ.

(11) Turning now to the inheritance of cousins, we notice that their regression may be represented by

$$x_1 = \tfrac{1}{4}h_1 + \tfrac{1}{4}k_0 \dots + \tfrac{1}{4}h_1{}' + \tfrac{1}{4}k_0{}' + x',$$

$$x_2 = \tfrac{1}{4}h_1{}'' + \tfrac{1}{4}k_0 \dots + \tfrac{1}{4}h_1{}''' + \tfrac{1}{4}k_0{}'' + x''.$$

Here h_1 and $h_1{}''$ are children of the same parents and have fraternal correlation; $h_1{}'$ and $h_1{}'''$ are their other parents, and without a double cousin marriage have no correlation with each other, or neglecting sexual selection with h_1 or $h_1{}''$; k_0 is the mid-parental system[§] of h_1, and therefore of $h_1{}''$; $k_0{}'$ and $k_0{}''$ the mid-parental systems of $h_1{}'$ and $h_1{}'''$, and accordingly, if there be no in-and-in breeding, uncorrelated with each other or with k_0.

Summing first for the array corresponding to $h_1, h_1{}''$,

$$S(x_1x_2) = n\{\tfrac{1}{16} h_1h_1{}'' + \tfrac{1}{16} k_0(h_1 + h_1{}'') + \tfrac{1}{16} k_0{}^2\},$$

---

[*] Mr. Galton took $r = 2r_1$; this is part of what, I think, the erroneous theory of regression developed in 'Natural Inheritance,' a theory which is inconsistent with the law of ancestral heredity given in the same work.

[†] 'Phil. Trans.' A, vol. 187, p. 281.

[‡] 'Phil. Trans.,' A, vol. 187, p. 284.

[§] This means that $k_0 = \frac{1}{2}k_2 + \frac{1}{4}k_3 + \frac{1}{8}k_4 + \dots$ where k_q is the common mid-qth parent of the two cousins.

where n is the number of pairs of cousins corresponding to h_1, h_1''. The factor 2 (see footnote, p. 404) does not occur here, as the cousins form parts of separate, and not identical, arrays. Now let us sum for all possible mid-parental systems, then if r' be the correlation of cousins and N the total number of cousin pairs :

$$N\sigma_0^2 r' = S[S(x_1 x_2)]$$
$$= \tfrac{1}{16}\{S(nh_1 h_1'') + S[nk_0(h_1 + h_1'')] + S(nk_0^2)\}.$$

But $S(nh_1 h_1'') = $ product moment for pairs of brothers $= N\sigma_0^2 r$.

$S(nk_0^2) = N\sigma_0^2 r$, by what precedes.

$S[nk_0(h_1 + h_1'')]$ is exactly the same as the sum of *all* offspring with the mid-parental system of ancestry beyond, since h_1 is not to be equal to h_1'',

$$= 2S[nh_1(\tfrac{1}{2}k_2 + \tfrac{1}{4}k_3 + \tfrac{1}{8}k_4 + \ldots)] \text{ for all values of } h_1.$$
$$= 2N(\tfrac{1}{2}\rho_1\sigma_0\Sigma_1 + \tfrac{1}{4}\rho_2\sigma_0\Sigma_2 + \tfrac{1}{8}\rho_3\sigma_0\Sigma_3 + \ldots),$$
$$= 2N\sigma_0^2(\tfrac{1}{2}ca^2 + \tfrac{1}{4}ca^4 + \tfrac{1}{8}ca^6 + \ldots),$$
$$= 2N\sigma_0^2 \frac{\tfrac{1}{2}ca^2}{1 - \tfrac{1}{2}a^2} = 0.4 \times N\sigma_0^2.$$

Thus $N\sigma_0^2 r' = \tfrac{1}{16}N\sigma_0^2(2r + 0.4)$,

and $r' = 0.075$.

Mr. Galton's value is $\frac{2}{27} = 0.074$ ('Natural Inheritance,' p. 133). Had we, however, applied his method correctly, considering cousins as the offspring of brothers, and adopted the value 0.3 given by his law of ancestral heredity for parent and offspring, we should have found 0.0360, instead of our present 0.075. Considering cousins as having two grandparents the same, we should have found 0.0450.

Second Cousins.—The correlated parts of their mid-parental systems are

$$\tfrac{1}{4}h_1 + \tfrac{1}{16}h_2 + \tfrac{1}{16}k_0,$$
$$\tfrac{1}{4}h_1' + \tfrac{1}{16}h_2' + \tfrac{1}{16}k_0,$$

where h_1 and h_1' are cousins, h_2 and h_2' brethren, and

$$k_0 = \tfrac{1}{2}k_3 + \tfrac{1}{4}k_4 + \tfrac{1}{8}k_5 + \ldots$$

is the mid-parental system of h_2 and h_2'.

In order to work out the correlation, we shall clearly want that of h_1 and h_2', or of nephew and uncle.

Here $x_1 = \tfrac{1}{4}h_2 + \tfrac{1}{4}k_0 + x' + \ldots$
$$x_2' = k_0 + x'',$$

give the correlated parts of the mid-parental systems.

Hence if r'' be the uncle-nephew correlation and N the total number of pairs

$$N\sigma_0^2 r'' = \tfrac{1}{4} S(nk_0 h_2) + \tfrac{1}{4} S(nk_0^2)$$

$$= \tfrac{1}{4}(0\cdot 2 + r) N \sigma_0^2,$$

since

$$S(nh_2 k_0) = \sigma_0 N (\tfrac{1}{2}\rho_1 \Sigma_1 + \tfrac{1}{4}\rho_2 \Sigma_2 + \ldots)$$

$$= \sigma_0^2 N (\tfrac{1}{2} ca^2 + \tfrac{1}{4} ca^4 + \ldots)$$

$$= \sigma_0^2 N \frac{\tfrac{1}{2} ca^2}{1 - \tfrac{1}{2} ca^2} = 0\cdot 2 \times \sigma_0^2 N.$$

Thus $r'' = 0\cdot 15$, or is double the correlation of first cousins. Here, as throughout, the variations of all generations, in this case those of uncles and nephews, have been treated as equal.

Returning to the correlation r''' of second cousins we have

$$N\sigma_0^2 r''' = \tfrac{1}{16}\Big\{ S(nh_1 h_1') + \tfrac{1}{16} S(nh_2 h_2') + \tfrac{1}{16} S(nk_0^2) + \tfrac{1}{4} S(nh_1 h_2')$$

$$+ \tfrac{1}{4} S(nh_1' h_2) + \tfrac{1}{2} S\Big(nk_0 \frac{h_1 + h_1'}{2} \Big) + \tfrac{1}{5} S\Big(nk_0 \frac{h_2 + h_2'}{2} \Big) \Big\}.$$

Evaluating each of these terms we have—

$$S(nh_1 h_1') = N r' \sigma_0^2 ; \quad S(nh_2 h_2') = N r \sigma_0^2 ; \quad S(nk_0^2) = N \sigma_0^2.$$

$S[n(h_1 h_2' + h_1' h_2)] = $ product moment of *all* uncles and nephews
$$= 2 N r'' \sigma_0^2.$$

$S[nk_0(h_1 + h_1')] = $ product moment of *all* offspring and the mid-
parental system of their grandfathers $= 2 S(nk_0 h_1)$ for all
values of h_1

$$= 2 S\Big[nh_1 \Big(\tfrac{1}{2} \frac{h_3 + h_3' m}{2} + \tfrac{1}{4} \frac{h_4 + h_4' + m(h_4'' + h_4''')}{4} + \ldots \Big) \Big]$$

$$= 2(\tfrac{1}{2} r_2 \sigma_0^2 N + \tfrac{1}{4} r_3 \sigma_0^2 N + \tfrac{1}{8} r_4 \sigma_0^2 N + \ldots)$$

$$= 2 N \sigma_0^2 (\tfrac{1}{2} ca^4 + \tfrac{1}{4} ca^6 + \tfrac{1}{8} ca^8 + \ldots) = 2 N \sigma_0^2 \frac{\tfrac{1}{2} ca^4}{1 - \tfrac{1}{2} a^2},$$

$$= 2 N \sigma_0^4 \times 0\cdot 1.$$

Similarly, $S[nk_0(h_2 + h_2')] = 2 N \sigma_0^2 \times 0\cdot 2$ as before (p. 408).

Thus finally: $r''' = \tfrac{1}{16}(r' + \tfrac{1}{16} r + \tfrac{1}{16} r + \tfrac{1}{2} r'' + \tfrac{1}{4} \times 0\cdot 2 + \tfrac{1}{16} \times 0\cdot 4)$

or
$$r''' = 0\cdot 0171875.$$

More distant collateral relationships, which can be found in like manner, and may be needed for the case of in-and-in breeding, say

from single pairs, are given in the table below. This case, which offers some striking applications of Galton's law, I postpone for the present.

Collateral Heredity according to Galton's Law.

Relationship.	Correlation.
Brethren......................	0·4000
Uncle and nephew	0·1500
Great uncle and nephew	0·0625
First cousins	0·0750
First cousins once removed	0·0344
Second cousins................	0·0172
Second cousins once removed....	0·0082
Third cousins.................	0·0041

Had we regarded second cousins as grandchildren of brethren, we should have found 0·0090 instead of 0·0172 for example, showing the degree of approximation of the incomplete theory.

(12) *On Cross Heredity.*—In my memoir on heredity cf 1895, I have defined *cross heredity* as the correlation between different organs in any two relations.* If we consider Galton's law of ancestral heredity to be applicable to the inheritance of any character or quality whatsoever, then we can obtain from it a solution of the whole problem of cross heredity. This solution seems so simple and plausible that it deserves careful consideration, and I hope shortly to be able to test it by the measurements in my possession.

Let A and B be any two relatives; 1 and 3 represent any two organs in A, 2 and 4 the same organs in B.

Now suppose we investigate the manner in which the index 1 to 3 is inherited by B, *i.e.*, let us find the correlation between the indices 1 to 3 and 2 to 4. Let ρ be the coefficient of heredity between the degrees of blood A and B, and suppose it by Galton's law to take the same value for all qualities and characters, then r will be the correlation not only between 1 and 2, and 3 and 4, but also between the indices 1 to 3 and 2 to 4. The value of this correlation was given by me in 'Roy. Soc. Proc.,' vol. 60, p. 493, Equation iv, and is

$$\rho = -\frac{r_{12}v_1v_2 - r_{14}v_1v_4 - r_{23}v_2v_3 + r_{34}v_3v_4}{\sqrt{v_1^2 + v_3^2 - 2r_{13}v_1v_3} \sqrt{v_2^2 + v_4^2 - 2r_{24}v_2v_4}}$$

where v_1, v_2, v_3, v_4 are the coefficients of variation of the four organs, and the r's are their coefficients of correlation.

Now if there be no secular selection $v_1 = v_2$, $v_3 = v_4$, and $r_{13} = r_{24}$;

* 'Phil. Trans.,' A, vol. 187, p. 259.

further, by Galton's law, $r_{12} = r_{34} = \rho$, for both are coefficients of direct heredity.

Hence

$$\rho = \frac{(v_1{}^2 + v_3{}^2)\rho - 2v_1v_3\dfrac{r_{14} + r_{23}}{2}}{v_1{}^2 + v_3{}^2 - 2v_1v_3\mathrm{R}},$$

where R is the organic correlation between the two organs in the same individual. Thus it follows at once that

$$\tfrac{1}{2}(r_{14} + r_{23}) = \rho \times \mathrm{R}.$$

Or the mean of the two coefficients of cross heredity is the product of the coefficient of direct heredity into the correlation of the two organs in the same individual. Now in all cases of interchangeable relationship, *i.e.*, brother and brother, or cousin and cousin, $r_{14} = r_{23}$, and it is highly probable that this is also true where the relationship is not interchangeable, *e.g.*, parent and offspring.* Thus we reach the exceedingly simple rule for cross heredity. *Multiply the coefficient of direct heredity by the coefficient of organic correlation, and we have the coefficient of cross heredity.*

For example, the organic correlation between femur and humerus is about 0·85 for Aino or French males. Hence we should expect to find the cross heredity between femur of parent and humerus of offspring to be about $0\cdot3 \times 0\cdot85 = 0\cdot25$. Thus Galton's law, even if it be not absolutely correct, will still serve as a useful standard to test the problems of cross heredity.

(13) *Conclusion.*—The above illustrations of Galton's law will suffice to prove the wide extent of its applications. If either that law, or its suggested modification, be substantially correct, they embrace the whole theory of heredity. They bring into one simple statement an immense range of facts, thus fulfilling the fundamental purpose of a great law of nature. It is true that there are difficulties which will have to be met, among which I would note two in particular:

(i) Galton's law makes the amount of inheritance an absolute constant for each pair of relatives. It would thus appear not to be a character of race or species, or one capable of modification by natural selection. This seems to me *a priori* to be improbable. I should imagine that greater or less inheritance of ancestral qualities might be a distinct advantage or disadvantage, and we should expect inheritance to be subject to the principle of evolution. This diffi-

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* For example, the correlation between the arm length of one brother and the stature of a second, must be equal to the correlation between the arm length of the second and the stature of the first. It is probable, but requires statistical confirmation, that the correlation between stature of parent and arm length of offspring is equal to the correlation between arm length of parent and stature of offspring.

culty would be to some extent met by introducing the coefficient γ, which I would propose to call the coefficient of heredity, and consider as capable of being modified with regard to both character and race. As such a law would cover Mr. Galton's case, there does not seem any objection to using the more general formula, until it is found that the strength of heredity is the same for all characters and races. Of course it may well be argued that heredity is something prior to evolution, itself determining evolution, and not determined by it. If this be so, its absolute fixity for all organs and races ought to be capable of observational proof.

(ii) For the inheritance of fertility in man from parent to offspring, Miss Alice Lee has recently worked out 6,000 male, and 4,000 female cases. The result shows that fertility is probably a heritable character, but the correlation between parent and offspring is scarcely one-tenth of that given by Galton's law. The difficulties of any fairly exact determination of the amount of fertility inherited in man under the present artificial conditions are very great, but even allowing for these, I think we must assert that fertility is inherited in man, but in a degree very much less than Galton's law would require.

I hold, then, that, as far as our knowledge goes at present, we must be cautious about treating γ as exactly equal to unity. That is a limiting value which certainly gives strikingly good results for a great deal of what is yet known, but we must wait at present for further determinations of hereditary influence, before the actual degree of approximation between law and nature can be appreciated. Even with regard to such determinations, there must be no haste to assert that they actually do contradict Galton's law. That law states the value of certain *partial* regression coefficients, the total regression coefficients that we have deduced from them are only correct on certain limiting hypotheses, the most important of which are the absence of reproductive selection, *i.e.*, the negligible correlation of fertility with the inherited character, and the absence of sexual selection. I propose to deal with the results of Galton's law, when assortative mating is taken into account, especially in the case of in-and-in breeding, in another paper. At present I would merely state my opinion that, with all due reservations, it seems to me that the law of ancestral heredity is likely to prove one of the most brilliant of Mr. Galton's discoveries; it is highly probable that it is the simple descriptive statement which brings into a single focus all the complex lines of hereditary influence. If Darwinian evolution be natural selection combined with *heredity*, then the single statement which embraces the whole field of heredity must prove almost as epoch-making to the biologist as the law of gravitation to the astronomer.

"Mathematical Contributions to the Theory of Evolution.
    On the Inheritance of the Cephalic Index." By Miss
    CICELY D. FAWCETT, B.Sc., and KARL PEARSON, M.A., F.R.S.,
    University College, London. Received January 27,—Read
    February 17, 1898.

(1) The cephalic index, when used to test any theory of heredity, possesses many merits, and at the same time one or two defects. In the first place it is supposed to be a marked racial character, and therefore might be considered to be strongly inherited. In the next place it remains sensibly constant after two years of age; thus the strength of inheritance can be ascertained by measurements on young children, whose parents are more frequently alive than if we have to wait for measurement till the offspring are of adult age. Further, although the cephalic index requires a more trained hand to measure it than some other measurements on the living subject, the trained observer will always deduce sensibly the same results;* on the other hand, stature measurements vary sensibly with the hour of the day, and with the observer. The need of a moderately trained observer is the chief defect of cephalic index measurements; it hinders the rapid collection of numerous family measurements; the difficulty, further, of satisfactorily measuring the female head without some derangement of the toilet is a further hindrance.† The merits of the cephalic index, however, as a test of heredity far surpass its demerits. A well-organised measurement of the cephalic index in pairs of relatives would probably give the best results available for the laws of inheritance. The cephalic index measured on the living head is of course not so satisfactory as that measured on the skull, but the latter may be considered, even with the aid of Röntgen rays, as at present quite out of the question. The following paper has been worked out, not on very good material or on material collected with the present end in view, but on the only material that seemed at present available. It suffices to justify the view that the inheritance of the cephalic index offers a most satisfactory method of testing the laws of heredity.

(2) Owing to the kindness of Mr. Francis Galton the Department of Applied Mathematics in University College, London, was placed in communication with Dr. Franz Boas, of the American Museum of Natural History, who is well known for his elaborate system of

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* This has been tested by frequent measurements of the same heads.

† The recent establishment of an anthropometric laboratory at Newnham College will, it may be hoped, remove the difficulty about head measurements on female students felt by the Cambridge Anthropometric Committee.

measurements on North American Indians. With extreme kindness
Dr. Boas* at once forwarded to England upwards of 1000 sheets of
measurements on comparable Indian tribes. These tribes, however,
contain extremely mixed blood. In the fewest cases were pure
Indian ancestors noted; one of the grandparents at least exhibited as
a rule European blood—English, Dutch, French, Irish, &c. Dr. Boas
himself writes:—

"I could not give you any series that was sufficiently extensive
and embraced pure Indians only, because among these tribes the
determination of relationships offers peculiar difficulties. I am afraid
that your results may also bring out the looseness of family
relations. I should not be surprised if the relation between father
and child were much lower than that between mother and child,
because often another person is actually the father of the child."

Dr. Boas's last surmise is amply verified; it will be found from the
table below that the coefficient of heredity between father and son is
abnormally small, while that between father and daughter is actually
less than the probable error of this series of measurements! If we
put upon one side any purely hypothetical supposition that illegiti-
mate births are more likely to be female than male, there would
seem reason to suppose some native custom by which it is held less
discreditable to pass off a daughter than a son upon the titular
husband. It may be asked whether, if the racial mixture is so great
and the paternity so obscure, it was worth while to undertake the
lengthy arithmetic† required to determine the hereditary correla-
tions. The answer is threefold: (a) if Galton's law of ancestral
heredity be correct, inheritance is not a racial character but a general
law of living forms, and racial mixtures will not influence the result;
(b) the results show that obscure paternity does not prevent good
values being found for other relationships; in fact, the fulfilment of
Dr. Boas's surmise is in itself not without value, as showing how
well our algebraic theory fits itself to the facts; it might almost be
said to provide a scientific measure of the conjugal fidelity of a race;
(c) it is always worth while to undertake an investigation on the
best material available, even if it be poor material for this pur-
pose, for it emphasizes the need of new and more elaborate obser-
vations.

(3) It will be seen from the table that it has only been possible to
determine the coefficient of heredity for small series, varying from
80 to 143 pairs of the seven relationships, four corresponding to the
first degree of direct kinship and three to the first degree of collateral

* It is difficult to sufficiently emphasize the disinterested service to science of
men who do not "monopolise" their anthropometric measurements.

† We have to thank Mr. Leslie Bramley Moore for much aid in extracting the
head measurements from the slips and calculating cephalic indices.

Inheritance of Cephalic Index—Table of Values.

Relation.	No.	Mean.	S. D.*	Coefficient of correlation.		
				Cephalic Index.	Stature.	Theory.
Fathers	131	80·55±0·18	3·064±0·128	[0·2245±0·0560]	0·3959 ±0·0259	0·3000
Sons	131	81·53±0·20	3·432±0·143			
Fathers	108	80·41±0·22	3·428±0·158	[0·0490±0·0647]	0·3603 ±0·0276	0·3000
Daughters	108	81·90±0·26	3·976±0·182			
Mothers	104	80·80±0·20	3·020±0·141	0·3696±0·0571	0·3018 ±0·0279	0·3000
Sons	104	81·55±0·23	3·524±0·165			
Mothers	82	80·88±0·28	3·843±0·202	0·3000±0·0603	0·2841 ±0·0292	0·3000
Daughters	82	81·53±0·31	4·143±0·218			
Brothers	139	80·57±0·19	3·652±0·136	0·3787±0·0490	0·3913 ±0·0232	0·4000
Brothers	139	81·42±0·21	3·765±0·152			
Brothers	143	81·58±0·20	3·490±0·139	0·3400±0·0499	0·3754 ±0·0170	0·4000
Sisters	143	81·38±0·20	3·588±0·143			
Sisters	80	82·10±0·27	3·686±0·194	0·4889±0·0574	0·4436 ±0·0222	0·4000
Sisters	80	81·84±0·16	4·069±0·217			

* S. D. = standard-deviation or "error of mean square."

kinship. The probable errors are, as might be expected from such small series, large. Putting aside the paternal relationship, we are justified in drawing certain general conclusions, which may be thus summed up :—

(a) The coefficients of heredity, as determined from the cephalic index, differ in all cases from those determined for stature by less than their probable error, and therefore by less than the probable error of their difference. The stature coefficients were obtained for the English middle classes.* We thus conclude that these results confirm Galton's law, in so far as they tend to show that the strength of *inheritance is not a character of race or organ.*

Cephalic index is clearly not more strongly inherited than stature. Its variability is also very much that of stature. It is accordingly difficult to see why it should be considered as peculiarly a racial character.

(b) The divergences between the observed values for the coefficients of inheritance for the cephalic index, and the theoretical values obtained on the basis of Galton's law of ancestral heredity, are greater than the divergences between the former and the coefficients for stature.† They do not, however, exceed the limits of errors of observation. In the case of mothers and sons the divergence is very slightly above the probable error; the observed and theoretical values are identical in the case of mothers and daughters; they are less than the probable error for brothers and brothers and only slightly larger than it for brothers and sisters; for sisters and sisters the divergence is about one and a half times the probable error. The mean weighted values of the coefficients for direct and collateral kinship are 0·3366 and 0·4004, the former differing by less than half its probable error from the theoretical value 0·3000, and the latter sensibly identical with its theoretical value, 0·4000.

We conclude, therefore, that Galton's law of ancestral heredity gives values for the inheritance within the limits of the probable errors of observation. But,

(c) As in the case of stature there is, on the whole, a tendency of the coefficients for cephalic index to be somewhat greater than their values as given by Galton's law. It is therefore reasonable to suppose that the heredity constant γ (introduced in the foregoing paper " On the Law of Ancestral Heredity ") is not, as Mr. Galton takes it, unity, but has some slightly less value.

Other conclusions which may be drawn from the above table are :

(d) Among Indians of mixed blood the women are more brachy-

* 'Phil. Trans.,' A, vol. 187, pp. 270 and 281.

† It is to be noted, that, putting paternity aside, the order of relative magnitude of the coefficients of heredity is precisely the same for both cephalic index and stature.

cephalic and more variable than the men. This is in accordance with the general conclusion reached in a paper on "Variation in Man and Woman," * namely:

"The lower races give us results in sensible accordance with those we have drawn from the data for ancient civilisations, namely, the women are on the whole more brachycephalic and slightly more variable than the men."

(e) The younger generation is more brachycephalic and more variable than its parentage.

The whole of this difference can hardly be due to any change of shape of the skull with old age, for the majority of parents had in this case not passed the prime of life. It may be due to (i) a correlation between dolichocephaly and fertility or between dolichocephaly and philogamy, or (ii) more probably to the action of natural selection (results obtained, but not yet published, by the present writers show a correlation between physique and cephalic index), or (iii) to a greater or less admixture of white blood in the younger generation.

(f) Parents of sons are significantly less variable than parents of daughters. This is in accordance with the result previously obtained that mediocre fathers are likely to have sons,† but disagrees with the result for stature—based on a far smaller probability—that mediocre mothers are likely to have daughters.

The conclusions of this paper, while appearing to the writers of interest, are to be taken, in the first place, as *suggestions* for much larger series of measurements and for new lines of investigation.

"Comparison of Oxygen with the Extra Lines in the Spectra of the Helium Stars, β Crucis, &c.; also Summary of the Spectra of Southern Stars to the $3\frac{1}{2}$ Magnitude and their Distribution." By FRANK McCLEAN, F.R.S. Received January 12,—Read February 3, 1898.

[PLATE 6.]

In a previous paper read before the Society on April 8, 1897, I suggested that the special lines present in spectra of the first division of helium stars (Type I, Divison 1a) might possibly be due to oxygen. These stars are associated by their position and distribution with the gaseous nebulæ, and some of the lines in their spectra correspond with bright lines observed by Campbell in nebulæ. The suggestion from this was that these stars are in the first stage of stellar development from gaseous nebulæ.

* Pearson, 'The Chances of Death,' vol. 1, p. 370.
† 'Phil. Trans.,' A, vol. 187, p. 274.

The special lines referred to are the extra lines which distinguish these spectra from those of the remaining helium stars of Division I*b*.

The indications in the spectra of the northern stars that these extra lines are due to oxygen are slight, as the lines at best are indistinct. Among the southern stars, however, there are several in the spectra of which these lines are better defined, and there is one, viz., β Crucis, in which they are very fairly defined.

The following stellar spectra are mounted on the accompanying plate, viz., κ Orionis, β Scorpii, β Canis Majoris, β Centauri, and β Crucis. These photographs are intended to show the gradual improvement in the definition of the extra lines, between κ Orionis and β Crucis, and to indicate their identity of origin throughout.

The extra lines in the spectrum of β Crucis are singled out by comparison with another helium star, viz., κ Argus, of Division I*b*, in which the extra lines do not appear. The lines are drawn out by themselves below the spectrum of β Crucis. They are then compared directly by juxtaposition with a drawing of the spectrum of oxygen as tabulated in the spectrum of air by Neovius (Stockholm, 1891, and Appendix E, 1894, of 'Watts's Index').

This comparison shows a close correspondence in the grouping of the extra lines with the spectrum of oxygen. The most remarkable correspondence is in the case of the large group on either side of Hδ. A slight shift of about a tenth metre is required to bring the groups into identical positions. However, the close similarity of the whole grouping of the two spectra as they appear on the plate admits of little doubt that the extra lines actually constitute the spectrum of oxygen. If this be established the spectrum of the first division of helium stars would be due to hydrogen, helium, and oxygen.

The scale attached to the spectra is based on standard lines that can be identified with certainty in the stellar spectra. It is interpolated between the standard lines. Its position in relation to the spectra is determined by the hydrogen lines. The wave-lengths employed are in accordance with Ångström's scale.

On the original negatives the distance between (H) and (F) measures about 1 inch. The negatives are enlarged about eight and a half times. It is difficult to fix the position of the lines—and especially of the hydrogen lines—on these enlargements with sufficient accuracy. A further correction than this would account for is however required in order to reduce the two spectra to exact coincidence. I believe it should be sought to some extent in a re-examination of the adopted wave-lengths of the hydrogen and of the oxygen spectra.

The spectrum of γ Argus is given on the plate in order to identify

it as a helium star. It contains two crucial lines of helium. The Wolf-Rayet stars, of which it is the principal example, are thus classified as helium stars. There are also some coincidences between the bright lines of γ Argus and the spectrum of oxygen, which suggest a possible connection.

The spectrum of μ Centauri is also given as a bright line helium star. The bright lines in this case are due to hydrogen, and the spectrum resembles that of γ Cassiopeiæ. The spectrum of δ Centauri is similar.

I take this opportunity of presenting a summary of the spectra of 116 stars to the 3½ magnitude in the Southern Hemisphere. They were photographed between May and October last by means of my own object-glass prism, mounted in front of the Cape astrographic telescope. This instrument, which is similar to my own telescope at Rusthall, with which the spectra of the northern stars were photographed, was kindly placed at my disposal by H.M. Astronomer, Dr. Gill. It may be a little time before the actual photographs of the stellar spectra are ready for presentation, and meanwhile the results are of interest.

In my previous paper I divided the sphere into eight equal areas consisting or two galactic equatorial areas and two galactic polar areas, situated on either side of the galactic equator. The northern stars already given occupy the upper or northerly lateral areas A, B, C, and D, also the southerly area AA. The southern stars now given occupy the lower or southerly lateral areas BB, CC, and DD. Their photographic spectra are distributed into these areas, and are classified on the same system as in the previous paper. The table of distribution for the whole sphere by areas and classes is given below.

There are in all 89 helium stars (Division I), distributed 71 in the galactic zones and 18 in the galactic polar areas, the areas being equal. There are 29 in the upper galactic zone (B and BB), and 42 in the lower galactic zone (C and CC). There are 9 in the upper polar areas (A and AA), and 9 in the lower polar areas (D and DD). There are 23 in the northerly halves of the two galactic zones (B and C) and 48 in the southerly halves (BB and CC).

The 81 stars in Division II, the Sirian stars, and Division III, the Procyon stars (which along with Division I constitute Secchi's Type I) are rather irregularly distributed throughout the sphere. There are 40 in the galactic zones and 41 in the galactic polar areas. There are 18 in the upper galactic zone (B and BB) and 22 in the lower (C and CC). There are 29 in the upper polar areas (A and AA) and 12 in the lower (D and DD). To the extent of the observations there is no condensation of stars of Divisions II and III in the galactic zones as there is in the case of stars of Division I.

The 106 stars in Divisions IV and V (II and III of Secchi's types)

are fairly evenly distributed throughout the sphere. There are 52 in the galactic zones and 54 in the galactic polar areas. There are 22 in the upper galactic zone (B and BB) and 30 in the lower (C and CC). There are 27 in the upper polar areas (A and AA) and 27 in the lower (D and DD).

The general distribution of the types of spectra throughout the sphere to the extent of the observations bears out generally the conclusion that stars with spectra of the more advanced types, in order of development, are evenly distributed in space. Also that stars with spectra more recent in order of development are mostly congregated in the galactic zones. The helium stars of Division I are predominant in the Southern Hemisphere, being congregated in the lower or southerly halves of the galactic zones (BB and CC). They include 48 stars out of a total of 94 stars in those areas. They are also more closely congregated in the vicinity of the galaxy than is the case in the northerly halves of the galactic zones. In the contiguous constellations of Musca, Crux, Centaurus, Lupus, and Scorpio there are 27 helium stars out of a total of 36 stars included in the tables. (The distribution of the helium stars throughout the sphere was illustrated by two small hand charts, not reproduced, on which these stars are coloured red.) Apparently the region in which the first stage of stellar development is now most active lies in the southerly half of the galaxy.

Table I.

Photographic Stellar Spectra—Stars to Magnitude $3\frac{1}{4}$.

Summary of Southern Stars—Regions BB, CC, and DD.

	Mag.	Div.	Area.		Mag.	Div.	Area.
Aquila.				Argo.			
λ	3·3	I (*b*)	CC	λ	2·5	IV	BB
				μ	2·9	IV	BB
Ara.				ν	3·5	I (*b*)	CC
α	2·9	I (*b*)	CC	ξ	3·4	IV	BB
β	2·8	IV	CC	π	2·7	IV	CC
γ	3·6	I (*a*)	CC	ρ	3·2	III	BB
ζ	3·2	IV	CC	σ	3·5	IV	CC
Argo.				τ	3·2	IV	CC
α	0·4	III	CC	υ	3·4	III	CC
β	2·0	II	CC	Canis Major.			
γ	3·0	I (*b*)	CC	α	−1·4	II	CC
δ	2·2	II	CC	β	2·0	I (*a*)	CC
ε	2·1	IV	CC	δ	1·9	IV	CC
ζ	2·5	I (*b*)	CC	ε	1·5	I (*a*)	CC
θ	2·9	I (*a*)	CC	ζ	3·0	I (*b*)	CC
ι	2·5	III	CC	η	2·4	I (*b*)	CC
κ	2·7	I (*b*)	CC	o²	3·0	I (*b*)	CC

Table I—*continued.*

	Mag.	Div.	Area.		Mag.	Div.	Area.
Capricornus.				**Libra.**			
β	3·4	IV	CC	σ (20)	3·2	V	BB
Centaurus.				**Lupus.**			
α	0·7	IV	CC	α	2·6	I (a)	BB
β	1·2	I (a)	BB	β	2·8	I (a)	BB
γ	2·4	II	BB	γ	3·2	I (a)	BB
δ	2·8	I (b)	BB	δ	3·7	I (a)	BB
ε	2·6	I (a)	BB	ε	3·7	I (b)	BB
ζ	2·7	I (b)	BB				
η	2·5	I (b)	BB	**Musca.**			
θ	2·7	IV	BB	α	2·9	I (b)	CC
ι	3·0	III	BB	β	3·4	I (b)	CC
κ	3·3	I (b)	BB				
λ	3·4	I (b)	CC	**Ophiuchus.**			
μ	3·4	I (a)	BB	β	2·9	IV	BB
Circinus.				ζ	2·8	I (a)	BB
α	3·5	III	CC	η	2·6	II	BB
				θ	3·4	I (b)	BB
Columba.				κ	3·4	IV	BB
α	2·7	I (b)	CC				
β	2·9	IV	CC	**Pavo.**			
Crux.				α	2·1	I (b)	DD
α	1·3	I (a)	BB	β	3·3	III	DD
β	1·7	I (a)	BB	δ	3·5	IV	DD
γ	2·0	V	BB				
δ	3·4	I (b)	BB	**Phœnix.**			
Doradus.				α	2·4	IV	DD
α	3·1	I (b)	DD	β	3·3	IV	DD
				γ	3·4	IV	DD
Eridanus.							
α	1·0	I (b)	DD	**Piscis Austr.**			
θ	2·6	II	DD	α	1·3	II	DD
φ	3·5	I (b)	DD				
χ	3·9	IV	DD	**Reticulum.**			
Grus.				α	3·3	IV	DD
α	1·9	I (b)	DD	**Sagittarius.**			
β	2·2	V	DD	γ²	3·0	IV	CC
γ	3·0	I (b)	DD	δ	2·8	IV	CC
ε	3·5	II	DD	ε	2·1	I (b)	CC
Hydrus.				ζ	2·9	II	CC
α	2·9	III	DD	η	3·0	V	CC
β	2·7	IV	DD	λ	3·1	IV	CC
γ	3·2	V	DD	π	3·1	III	CC
				σ	2·3	I (b)	CC
Indus.				φ	3·3	I (b)	CC
α	3·1	IV	DD	**Scorpio.**			
				α	1·1	V	BB
Lepus.				β'	2·9	I (a)	BB
α	2·7	III	CC	δ	2·5	I (a)	BB
β	3·0	IV	CC	ε	2·2	IV	BB
ε	3·3	IV	CC	θ	2·1	III	CC
μ	3·3	I (b)	CC	ι'	3·3	III	CC
				κ	2·6	I (a)	CC
				λ	1·7	I (a)	CC

Table I—*continued.*

	Mag.	Div.	Area.		Mag.	Div.	Area.
Scorpio.				Telescopium.			
μ	3·6	I (*a*)	BB	*a*	3·5	I (*b*)	CC
π	3·1	I (*a*)	BB				
σ	3·0	I (*a*)	BB	Toucan.			
τ	2·9	I (*a*)	BB	*a*	2·8	IV	DD
υ	2·8	I (*b*)	CC				
				Triangulum.			
Serpens.				*a*	2·2	IV	CC
η	3·4	IV	BB	β	3·1	III	CO
				γ	3·1	II	CC

NOTE.—The magnitudes are taken from the 'Nautical Almanac' (or from Gould).

Table II.

Summary Tables of Distribution of Gaseous Nebulæ and of Stellar Types.   Stars to the 3½ Magnitude.

Table No. 1.

Stellar Types.	A.	B.	C.	D.	Total.	AA.	BB.	CC.	DD.	Total.
Planetary nebulæ	2	3	8	2	(15)	2	7	3	0	(12)
Extended nebulæ......	1	4	8	4	(17)	1	4	3	1	(9)
Total gaseous nebulæ ..	3	7	16	6	(32)	3	11	6	1	(21)

NOTE.—Extracted from Table in Frost's edition of 'Scheiner's Astronomical Spectroscopy.'

Table No. 2.

Stellar Types.	A.	B.	C.	D.	Total.	AA.	BB.	CC.	DD.	Total.
Division I	3	6	17	3	(29)	6	23	25	6	(60)
„ II	10	7	0	3	(20)	3	2	5	3	(13)
„ III	7	8	8	4	(27)	9	1	9	2	(21)
„ IV	14	8	9	13	(44)	9	9	16	9	(43)
„ V	1	2	4	3	(10)	3	3	1	2	(9)
	35	31	38	26	(130)	30	38	56	22	(146)

A.

rucis, &c. (Div. I*a*).

pe 1, Div. 1a. 1	*k* Orionis	2.4
2	β Scorpii	2.9
3	β Can. Maj.	2.0
4	β Centauri	1·2
5	β Crucis	1.7
ype 1, Div. 1b. 6	γ Argus	3.0
	Cleveite Gas.	
ype 1, Div. 1b. 7	μ Centauris	3·4
8	*k* Argus	2.7
ype 1, Div. 1a. 9	β Crucis	1.7
	Extra Lines	
	Oxygen	

Table No. 3.

	A.	B.	C.	D.	Total.	AA.	BB.	CC.	DD.	Total.
Stellar Types.										
Division I	3	6	17	3	(29)	6	23	25		(60)
„ II and III	17	15	8	7	(47)	12	3	14	5	(34)
„ IV and V	15	10	13	16	(54)	12	12	17	11	(52)
	35	31	38	26	(130)	30	38	56	22	(146)

Table No. 4.

	A and AA.	B and BB.	C and CC.	D and DD.	Total.
Stellar Types.					
Division I	9	29	42	9	(89)
„ II and III	29	18	22	12	(81)
„ IV and V	27	22	30	27	(106)
	65	69	94	48	(276)

February 24, 1898.

Sir JOHN EVANS, K.C.B., D.C.L., LL.D., Treasurer, in the Chair.

Meeting for Discussion.

Subject :—The Scientific Advantages of an Antarctic Expedition.

The Discussion was opened with a communication by Dr. John Murray, and the following gentlemen contributed remarks :—The Duke of Argyll, Sir J. D. Hooker, Dr. Nansen, Dr. G. Neumayer of Hamburg, Sir Clements Markham, Dr. A. Buchan, Sir A. Geikie, Dr. Sclater, Professor D'Arcy Thompson, Admiral Sir W. J. L. Wharton.

"The Scientific Advantages of an Antarctic Expedition." By
JOHN MURRAY, D.Sc., LL.D., Ph.D., F.R.S. Received
January 25,—Read February 24, 1898.

From a scientific point of view the advantages to be derived from
a well-equipped and well-directed expedition to the Antarctic would,
at the present time, be manifold. Every department of natural
knowledge would be enriched by systematic observations as to
the order in which phenomena coexist and follow each other in
regions of the earth's surface about which we know very little or
are wholly ignorant. It is one of the great objects of science to
collect observations of the kind here indicated, and it may be safely
said that without them we can never arrive at a right understanding
of the phenomena by which we are surrounded, even in the habitable
parts of the globe.

Before considering the various orders of phenomena, concerning
which fuller information is urgently desired, it may be well to point
out a fundamental topographical difference between the Arctic and
Antarctic. In the northern hemisphere there is a polar sea almost
completely surrounded by continental land, and continental conditions
for the most part prevail. In the southern hemisphere, on the other
hand, there is almost certainly a continent at the South Pole, which
is completely surrounded by the ocean, and, in those latitudes, the
most simple and extended oceanic conditions on the surface of the
globe are encountered.

The Atmosphere.

One of the most remarkable features in the meteorology of the
globe is the low atmospheric pressure at all seasons in the southern
hemisphere south of latitude 45° S., with the accompanying strong
westerly and north-westerly winds, large rain- and snow-fall, all
round the South Polar regions. The mean pressure seems to be less
than 29 inches, which is much lower than in similar latitudes in the
northern hemisphere. Some meteorologists hold that this vast
cyclonic system and low-pressure area continues south as far as the
pole, the more southerly parts being traversed by secondary cyclones.
There are, however, many indications that the extreme South Polar
area is occupied by a vast anticyclone, out of which winds blow
towards the girdle of low pressure outside the ice-bound region. In
support of this view it is pointed out that Ross's barometric observa-
tions indicate a gradual rise in the pressure south of the latitude of
75° S., and all Antarctic voyagers agree that when near the ice the

majority of the winds are from the south and south-east, and bring clear weather with fall of temperature, while northerly winds bring thick fogs with rise of temperature.

All our knowledge of the meteorological conditions of the Antarctic is limited to a few observations during the midsummer months, and these indicate that the temperature of the snow-covered Antarctic continent is even at that time much lower than that of the surrounding sea. The anticyclonic area at the South Pole appears therefore to be permanent, and when in winter the sea-ice is for the most part continuous and extends far to the north, the anticyclonic area has most probably a much wider extension than in summer. This is indicated by the south-easterly winds which at times blow towards the southern point of the American continent in June and July.

All observations in high southern latitudes indicate an extremely low summer temperature. In winter we have no direct observations. The mean of Ross's air temperatures south of latitude 63° S. was 28·74° F., which is about the freezing point of sea-water, and his maximum temperature was 43·5° F. Both Wilkes and D'Urville observed pools of fresh water on several icebergs, and, when sailing along the ice barrier, Ross saw "gigantic icicles depending from every projecting point of its perpendicular cliffs,"* so it is probable that extensive melting sometimes takes place.

In the latitude of the Antarctic circle the air is frequently at or near the point of saturation, and precipitation takes place in the form of rain, sleet, snow, or hail. Most of the observations near the ice-covered land show, however, a much drier atmosphere, and in all probability precipitation over the Antarctic continent takes place in the form of fine snow crystals, such as is recorded in the interior of Greenland.

There would appear, then, to be good reasons for believing that the region of the South Pole is covered by what may be regarded practically as a great permanent anticyclone, with a much wider extension in winter than in summer. It is most likely that the prevailing winds blow out from the pole all the year round towards the surrounding sea, as in the case of Greenland, but, unlike Greenland, this area is probably seldom traversed by cyclonic disturbances.

But what has been stated only shows how little real knowledge we possess concerning the atmospheric conditions of high southern latitudes. It is certain, however, that even two years' systematic observations within these regions would be of the utmost value for the future of meteorological science.

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* Ross, ' Antarctic Voyage,' vol. 1, p. 237.

Antarctic Ice.

From many points of view it would be important to learn some-
thing about the condition and distribution of Antarctic sea-ice during
the winter months, and especially about the position and motions of the
huge table-shaped icebergs at this and other seasons of the year. These
flat-topped icebergs, with a thickness of 1200 or 1500 feet, with their
stratification and their perpendicular cliffs, which rise 150 or 200
feet above and sink 1100 or 1400 feet below the level of the sea,
form the most striking peculiarity of the Antarctic Ocean. Their
form and structure seem clearly to indicate that they were formed
on an extended land surface, and have been pushed out over low-lying
coasts into the sea.

Ross sailed for 300 miles along the face of a great ice-barrier from
150 to 200 feet in height, off which he obtained depths of 1800 and
2400 feet. This was evidently the sea-front of a great creeping
glacier or ice-cap just then in the condition to give birth to the
table-shaped icebergs, miles in length, which have been described by
every Antarctic voyager.

All Antarctic land is not, however, surrounded by such inacces-
sible cliffs of ice, for along the seaward faces of the great mountain
ranges of Victoria Land the ice and snow which descend to the sea
apparently form cliffs not higher than 10 to 20 feet, and in 1895 Kris-
tensen and Borchgrevink landed at Cape Adare on a pebbly beach,
occupied by a penguin rookery, without encountering any land-ice
descending to the sea. Where a penguin rookery is situated, we may
be quite sure that there is occasionally open water for a considerable
portion of the year, and that consequently landing might be effected
without much difficulty or delay, and further that a party, once
landed, might with safety winter at such a spot, where the penguins
would furnish an abundant supply of food and fuel. A properly
equipped party of observers situated at a point like this on the
Antarctic continent for one or two winters might carry out a most
valuable series of scientific observations, make successful excursions
towards the interior, and bring back valuable information as to the
probable thickness of the ice-cap, its temperature at different levels,
its rate of accumulation, and its motions, concerning all which points
there is much difference of opinion among scientific men.

Antarctic Land.

Is there an Antarctic continent? It has already been stated that
the form and structure of the Antarctic icebergs indicate that they
were built up on, and had flowed over, an extended land surface. As
these bergs are floated to the north and broken up in warmer lati-

tudes they distribute over the floor of the ocean a large quantity of glaciated rock fragments and land detritus. These materials were dredged up by the "Challenger" in considerable quantity, and they show that the rocks over which the Antarctic land-ice moved were gneisses, granites, mica-schists, quartziferous diorites, grained quartzites, sandstones, limestones, and shales. These lithological types are distinctively indicative of continental land, and there can be no doubt about their having been transported from land situated towards the South Pole. D'Urville describes rocky islets off Adélie Land composed of granite and gneiss. Wilkes found on an iceberg, near the same place, boulders of red sandstone and basalt. Borchgrevink and Bull have brought back fragments of mica-schists and other continental rocks from Cape Adare. Dr. Donald brought back from Joinville Island a piece of red jasper or chert containing Radiolaria and Sponge spicules. Captain Larsen brought from Seymour Island pieces of fossil coniferous wood, and also fossil shells of *Cucullœa, Cytherea, Cyprina, Teredo,* and *Natica,* having a close resemblance to species known to occur in lower Tertiary beds in Britain and Patagonia. These fossil remains indicate in these areas a much warmer climate in past times. We are thus in possession of abundant indications that there is a wide extent of continental land within the ice-bound regions of the southern hemisphere.

It is not likely that any living land-fauna will be discovered on the Antarctic continent away from the penguin rookeries. Still, an Antarctic expedition will certainly throw much light on many geological problems. Fossil finds in high latitudes are always of special importance. The pieces of fossil wood from Seymour Island can hardly be the only relics of plant life that are likely to be met with in Tertiary and even older systems within the Antarctic. Tertiary, Mesozoic, and Palæozoic forms are tolerably well developed in the Arctic regions, and the occurrence of like forms in the Antarctic regions might be expected to suggest much as to former geographical changes, such as the extension of Antarctica towards the north, and its connexion with, or isolation from, the northern continents, and also as to former climatic changes, such as the presence in pre-Tertiary times of a uniform temperature in the waters of the ocean all over the surface of the globe.

Magnetic and Pendulum Observations, Geodetic Measurements, Tides, and Currents.

In any Antarctic expedition magnetic observations would, of course, form an essential part of the work to be undertaken, and the importance of such observations has been frequently dwelt upon by eminent physicists and navigators. Should a party of competent

observers be stationed at Cape Adare for two years, pendulum obser-
vations could be carried out there and at other points within the
Antarctic, or even on the ice-cap and icebergs. It might be possible
to measure a degree on the Antarctic continent or ice-cap, which
would be a most useful thing to do. By watching the motions of
the icebergs and ice from land at Cape Adare much would be learnt
about oceanic currents, and our knowledge of the tides would be
increased by a systematic series of tidal observations on the shores
of the Antarctic continent, where we have at present no observa-
tions. The series of scientific observations here mentioned, and
others that might be indicated, would fill up many gaps in our
knowledge of the physical conditions of these high southern
latitudes.

Depth of the Antarctic Ocean.

In regard to the depth of the ocean immediately surrounding the
Antarctic continent we have at present very meagre information,
and one of the objects of an Antarctic expedition would be to supple-
ment our knowledge by an extensive series of soundings in all
directions throughout the Antarctic and Southern Oceans. It would
in this way be possible, after a careful consideration of the depths
and marine deposits, to trace out approximately the outlines of the
Antarctic continent. At the present time we know that Ross
obtained depths of 100 to 500 fathoms all over the great bank
extending to the east of Victoria Land, and somewhat similar
depths have been obtained extending for some distance to the east of
Joinville Island. Wilkes sounded in depths of 500 and 800 fathoms
about 20 or 30 miles off Adélie Land. The depths found by the
" Challenger " in the neighbourhood of the Antarctic circle were
from 1300 to 1800 fathoms, and further north the " Challenger "
soundings ranged from 1260 to 2600 fathoms. To the south-west of
South Georgia, Ross paid out 4000 fathoms of line without reaching
bottom. In the charts of depth which I have constructed I have
always placed a deep sea in this position, for it appears to me that
Ross, who knew very well how to take soundings, was not likely to
have been mistaken in work of this kind.

The few indications which we thus possess of the depth of the
ocean in this part of the world seem to show that there is a gradual
shoaling of the ocean from very deep water towards the Antarctic
continent, and, so far as we yet know, either from soundings or tem-
perature observations, there are no basins cut off from general
oceanic circulation by barriers or ridges, similar to those found
towards the Arctic.

Deposits of the Antarctic Ocean.

The deposits which have been obtained close to the Antarctic continent consist of blue mud, containing glauconite, made up for the most part of detrital matters brought down from the land, but containing a considerable admixture of the remains of pelagic and other organisms. Further to the north there is a very pure diatom ooze, containing a considerable quantity of detrital matter from icebergs, and a few pelagic foraminifera. This deposit appears to form a zone right round the earth in these latitudes. Still further to the north the deposits pass in deep water, either into a Globigerina ooze, or into a red clay with manganese nodules, sharks' teeth, ear-bones of whales, and the other materials characteristic of that deep-sea deposit. Since these views, however, as to the distribution of deep-sea deposits throughout these high southern latitudes, are founded upon relatively few samples, it cannot be doubted that further samples from different depths in the unexplored regions would yield most interesting information.

Temperature of the Antarctic Ocean.

The mean daily temperature of the surface waters of the Antarctic, as recorded by Ross, to the south of latitude 63° S. in the summer months, varies from 27·3° to 33·6°, and the mean of all his observations is 29·85°. As already stated, his mean for the air during the same period is somewhat lower, being 28·74°. In fact, all observations seem to show that the surface water is warmer than the air during the summer months.

The "Challenger" observations of temperature beneath the surface indicate the presence of a stratum of colder water wedged between warmer water at the surface, and warm water at the bottom. This wedge-shaped stratum of cold water extends through about 12° of latitude, the thin end terminating about latitude 53° S., its temperature varying from 28° at the southern thick end to 32·5° at the northern thin end, while the temperature of the overlying water ranges from 29° in the south to 38° in the north, and that of the underlying water from 32° to 35°. This must be regarded as the distribution of temperature only during the summer, for it is improbable that during the winter months there is a warmer surface layer.

In the greater depths of the Antarctic, as far south as the Antarctic circle, the temperature of the water varies between 32° and 35° F., and is not, therefore, very different from the temperature of the deepest bottom water of the tropical regions of the ocean. The presence of this relatively warm water in the deeper parts of the

Antarctic Ocean may be explained by a consideration of general oceanic circulation. The warm tropical waters which are driven southwards along the eastern coasts of South America, Africa, and Australia, into the great all-encircling Southern Ocean, there become cooled as they are driven to the east by the strong westerly winds. These waters, on account of their high salinity, can suffer much dilution with Antarctic water, and still be denser than water from these higher latitudes at the same temperature. Here the density observations and the sea-water gases indicate that the cold water found at the greater depths of the ocean probably leaves the surface and sinks towards the bottom in the Southern Ocean, between the latitudes of 45° and 56° S. These deeper, but not necessarily bottom, layers are then drawn slowly northwards towards the tropics, to supply the deficiencies there produced by evaporation and southward-flowing surface currents, and these deeper layers of relatively warm water appear likewise to be slowly drawn southwards to the Antarctic area to supply the place of the ice-cold currents of surface water drifted to the north. This warm underlying water is evidently a potent factor in the melting and destruction of the huge table-topped icebergs of the southern hemisphere. While these views as to circulation appear to be well established, still a fuller examination of these waters is most desirable at different seasons of the year, with improved thermometers and sounding machines. Indeed, all deep-sea apparatus has been so much improved as a result of the " Challenger " explorations, that the labour of taking specific gravity and all other oceanographical observations has been very much lessened.

Pelagic Life of the Antarctic Ocean.

In the surface waters of the Antarctic there is a great abundance of diatoms and other marine algæ. These floating banks or meadows form primarily not only the food of pelagic animals, but also the food of the abundant deep-sea life which covers the floor of the ocean in these south polar regions. Pelagic animals, such as copepods, amphipods, molluscs, and other marine organisms, are also very abundant, although species are fewer than in tropical waters. Some of these animals seem to be nearly, if not quite, identical with those found in high northern latitudes, and they have not been met with in the intervening tropical zones. The numerous species of shelled Pteropods, Foraminifera, Coccoliths and Rhabdoliths, which exist in the tropical surface waters, gradually disappear as we approach the Antarctic circle, where the shelled Pteropods are represented by a small *Limacina*, and the Foraminifera by only two species of *Globigerina*, which are apparently identical with those in the Arctic Ocean. A peculiarity of the tow-net gatherings made by the " Challenger "

Expedition in high southern latitudes, is the great rarity or absence of the pelagic larvæ of benthonic organisms, and in this respect they agree with similar collections from the cold waters of the Arctic seas. The absence of these larvæ from polar waters may be accounted for by the mode of development of benthonic organisms, to be referred to presently. It must be remembered that many of these pelagic organisms pass most of their lives in water of a temperature below 32° F., and it would be most interesting to learn more about their reproduction and general life-history.

Benthos Life of the Antarctic Ocean.

At present we have no information as to the shallow-water fauna of the Antarctic continent; but, judging from what we do know of the off-lying Antarctic islands, there are relatively few species in the shallow waters in depths less than 25 fathoms. On the other hand, life in the deeper waters appears to be exceptionally abundant. The total number of species of Metazoa collected by the "Challenger" at Kerguelen in depths less than 50 fathoms was about 130, and the number of additional species known from other sources from the shallow waters of the same island is 112, making altogether 242 species, or thirty species less than the number obtained in eight deep hauls with the trawl and dredge in the Kerguelen region of the Southern Ocean, in depths exceeding 1260 fathoms, in which eight hauls 272 species were obtained. Observations in other regions of the Great Southern Ocean, where there is a low mean annual temperature, also show that the marine fauna around the land in high southern latitudes appears to be very poor in species down to a depth of 25 fathoms, when compared with the number of species present at the mud-line about 100 fathoms, or even at depths of about 2 miles.

In the year 1841 Sir James Clark Ross dredged off the Antarctic continent species which he recognised as the same as he had been in the habit of taking in equally high northern latitudes, and he suggested that they might have passed from the one pole to the other by way of the cold water of the deep sea. Subsequent researches show that, as with pelagic organisms, many of the bottom-living species are identical with, or closely allied to, those of the Arctic regions, and are not represented in the intermediate tropical areas. For instance, the most striking character of the shore-fish fauna of the Southern Ocean is the reappearance of types inhabiting the corresponding latitudes of the northern hemisphere, and not found in the intervening tropical zone. This interruption of continuity in the distribution of shore-fishes is exemplified by species as well as genera, and Dr. Günther enumerates eleven species and twenty-nine genera as illustrating this method of distribution.

The following are the eleven species:—*Chimæra monstrosa, Galeus canis, Acanthias vulgaris, Acanthias Blainvilli, Rhina squatina, Zeus faber, Lophius piscatorius, Centriscus scolopax, Engraulis encrasicholus, Clupea sprattus, Conger vulgaris.*

The genus by which the family Berycidæ is represented in the southern temperate zone (*Trachichthys*) is much more nearly allied to the northern than to the tropical genera. " As in the northern temperate zone, so in the southern . . . . the variety of forms is much less than between the tropics. This is especially apparent on comparing the number of species constituting a genus. In this zone, genera composed of more than ten species are the exception, the majority having only from one to five." . . . . "*Polyprion* is one of those extraordinary instances in which a very specialised form occurs at almost opposite points of the globe, without having left a trace of its previous existence in, or of its passage through, the intermediate space."

Speaking of the shore-fishes of the Antarctic Ocean, Günther says : " The general character of the fauna of Magelhaen's Straits and Kerguelen's Land is extremely similar to that of Iceland and Greenland. As in the Arctic fauna, Chondropterygians are scarce, and represented by *Acanthias vulgaris* and species of *Raja* . . . . As to Acanthopterygians, Cataphracti, and Scorpænidæ are represented as in the Arctic fauna, two of the genera (*Sebastes* and *Agonus*) being identical. The Cottidæ are replaced by six genera of Trachinidæ, remarkably similar in form to Arctic types . . . . Gadoid fishes reappear, but are less developed; as usual they are accompanied by *Myxine*. The reappearance of so specialised a genus as *Lycodes* is most remarkable."[*]

These statements with reference to shore-fishes might, with some modifications, be repeated concerning the distribution and character of all classes of marine invertebrates in high northern and high southern latitudes.[†] The " Challenger " researches show that nearly

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[*] Günther, ' Study of Fishes,' pp. 282—290. Edinburgh. 1880.

[†] Ortmann, speaking of the Decapod Crustacea, says : " Nach dem Stande unserer jetzigen Kenntniss ist *keine einzige bipolare Art bekannt* " (Ortmann, ' Zoologische Jahrbücher,' Abth. f. Syst., &c., Bd. ix, p. 585, 1896). Henderson, in his report on the " Challenger " Ancmura, in describing *Lithodes murrayi* from the Kerguelen region, says it " is apparently most closely allied to *Lithodes maia* " (from the North Atlantic), " but the latter species is of large size, and the spines on the carapace are more numerous and more uniformly equal in size " (Henderson, ' Zool. Chall. Exp.,' pt. 69, p. 44). Henderson writes me that these very slight differences were the only ones he could detect, and it seems evident that had the two specimens been taken from the same haul of the trawl, or from the same locality, they would never have been erected into two distinct species. Henderson writes me further that throughout the entire range of Crustacea there is no better illustration of bipolarity than that furnished by the Lithodidæ. For instances of

250 species taken in high southern latitudes occur also in the northern hemisphere, but are not recorded from the tropical zone. Fifty-four species of sea-weed shave also been recorded as showing a similar distribution.* Bipolarity in the distribution of marine organisms is a fact, however much naturalists may differ as to its extent and the way in which it has originated.

All those animals which secrete large quantities of carbonate of lime greatly predominate in the tropics, such as Corals, Decapod Crustacea, Lamellibranchs, and Gasteropods. On the other hand, those animals in which there is a feeble development of carbonate of lime structures predominate in cold polar waters, such as Hydroida, Holothurioidea, Annelida, Amphipoda, Isopoda, and Tunicata. This difference is in direct relation with the temperature of the water in which these organisms live, a much more rapid and abundant precipitate of carbonate of lime being thrown down in warm than in cold water by ammonium carbonate, one of the waste products of organic activity.

In the Southern and Sub-antarctic Ocean a large proportion of the Echinoderms develop their young after a fashion which precludes the possibility of a pelagic larval stage. The young are reared within or upon the body of the parent, and have a kind of commensal connection with her till they are large enough to take care of themselves. A similar method of direct development has been observed in eight or nine species of Echinoderms from the cold waters of the northern hemisphere. On the other hand, in temperate and tropical regions the development of a free-swimming larva is so entirely the rule that it is usually described as the normal habit of the Echinodermata. This similarity in the mode of development between Arctic and Antarctic Echinoderms (and the contrast to what takes place in the tropics) holds good also in other classes of Invertebrates, and probably accounts for the absence of free-swimming larvæ of benthonic animals in the surface gatherings in Arctic and Antarctic waters.

What is urgently required with reference to the biological problems here indicated is a fuller knowledge of the facts, and it cannot be doubted that an Antarctic expedition would bring back collections and observations of the greatest interest to all naturalists and physiologists, and without such information it is impossible to discuss with success the present distribution of organisms over the surface of the globe, or to form a true conception of the antecedent conditions by which that distribution has been brought about.

new species being made from purely geographical considerations, see 'Summary of Results, "Challenger" Expedition,' p. 1440-45.

* Murray and Barton, 'Phycological Memoirs of the British Museum,' part iii. London. 1895.

Concluding Remarks.

There are many directions in which an Antarctic Expedition would carry out important observations besides those already touched on in the foregoing statement. From the purely exploratory point of view much might be urged in favour of an Antarctic Expedition at an early date; for the further progress of scientific geography it is essential to have a more exact knowledge of the topography of the Antarctic regions. This would enable a more just conception of the volume relations of land and sea to be formed, and in connexion with pendulum observations some hints as to the density of the sub-oceanic crust and the depth of ice and snow on the Antarctic continent might be obtained. In case the above sketch may possibly have created the impression that we really know a great deal about the Antarctic regions, it is necessary to re-state that all the general conclusions that have been indicated are largely hypo-thetical, and to again urge the necessity for a wider and more solid base for generalisations. The results of a successful Antarctic Expedition would mark a great advance in the philosophy—apart from the mere facts—of terrestrial science.

No thinking person doubts that the Antarctic will be explored. The only questions are: when? and by whom? I should like to see the work undertaken at once, and by the British Navy. I should like to see a sum of £150,000 inserted in the Estimates for the purpose. The Government may have sufficient grounds for declining to send forth such an expedition at the present time, but that is no reason why the scientific men of the country should not urge that the exploration of the Antarctic would lead to important additions to knowledge, and that, in the interests of science among English speaking peoples, the United Kingdom should take not only a large but a leading part in any such exploration.

Remarks by the Duke of Argyll.

Scientific men generally feel, I think, that they do not need to give detailed reasons in connexion with particular subjects of inquiry, to justify their unanimous desire for an Antarctic Expedition. It is enough, surely, for them to point out the fact that a very large area of the surface of our small planet is still almost unknown to us. That it should be so seems almost a reproach to our civilisation. As to detailed reasons, it may almost be said with truth that there is hardly one of the physical sciences on which important light may not

be cast by Antarctic exploration. Oceanic circulation; meteorology; magnetism; distribution of animal and vegetable life, not only in the present but in the past; geology; mineralogy; volcanic action under special conditions—all of these are subjects on which the phenomena of the Antarctic regions are sure to bear directly.

If, however, I am asked to specify more particularly the question on which I look for invaluable evidence which can be got nowhere else, I must name, above all others, the most difficult questions involved in quaternary geology. Geologists are nearly all agreed that there has been, very recently, a glacial age—an age in which glacial conditions prevailed over the whole northern hemisphere to a much lower latitude than they prevail now. But geologists differ widely and fundamentally from each other as to the form which glacial agencies took during that period. In particular, many geologists believe in what they call an " ice sheet "—that is to say, in the northern world having been covered by an enormous mass of ice several thousand feet thick, which, as they assert, "flowed" over mountain areas as well as over plains, and filled up the bed of seas of a considerable depth. Other geologists disbelieve in this agency altogether. They deny that even if such a body of ice ever existed, it could possibly have moved in the way which the theory assumes. They affirm, also, that the facts connected with glaciated surfaces do not indicate the planing down by one universal sheet of enormous weight and pressure; but, on the contrary, the action or small and lighter bodies of ice, which have acted partially and unequally on different surfaces differently exposed.

We might have hoped that this controversy could be settled by the facts connected with the only enormous ice-sheet which exists in the Northern Hemisphere, viz., that which covers the great continent of Greenland. But that ice-sheet, enormous though it be, does certainly not do what the ice-sheet of the Glacial Age is supposed to have done. That is to say, it does not flow out from Greenland, fill the adjacent seas, or override the opposite coasts, even in so narrow a strait as Smith's Sound. But this evidence is negative only. In the Antarctic continent we have reason to believe that there is a larger ice-sheet, and it certainly does protrude into the adjacent seas, not merely by sending out vast floating fragments, but in unbroken ice cliffs of great height. Now we want to know exactly under what conditions this protrusion takes place. Dr. Murray speaks of it as "creeping" seawards—a more cautious word than "flowing." But is it certain that it does even creep? May it not simply grow by accretion or aggregation till it reaches a depth of water so great as to break it off by floatation? Does it, or does it not, carry detritus when no detritus has been dropped on its surface? Or does it pick up detritus from its own bed? Or does

it push foreign matter before it? Is the perfectly tabular form
of the Antarctic icebergs compatible with any differential movement
in the parent mass at all; or does it not indicate, on the contrary, a
condition of immobility until their buoyancy lifts great fragments
off? What is the condition of the rocks on which they rest? Is
there any thrust upon the mass from the mountain ranges on which
the gathering ground lies? Or is the whole country one vast gather-
ing ground from the continual excess of precipitation over melting?
These questions, and a hundred others, have to be solved by
Antarctic discovery; and until they are solved we cannot argue
with security on the geological history of our own now temperate
regions. The Antarctic continent is unquestionably the region of
the earth in which glacial conditions are at their maximum, and
therefore it is the region in which we must look for all the informa-
ation attainable towards, perhaps, the most difficult problem with
which geological science has to deal.

Remarks by Sir J. D. Hooker.

Dr. Murray's admirable summary of the scientific information
obtainable by an organised exploration of the Antarctic regions
leaves nothing further to be said under that head. I can only
record the satisfaction with which I heard it, and my earnest hope
that it will lead to action being taken by the Government in the
direction indicated.

Next to a consideration of the number and complexity of the
objects to be attained by an Antarctic expedition, what dwells most
in my imagination is the vast area of the unknown region which is to
be the field for investigation—a region which in its full extension
reaches from the latitude of 60° S. to the Southern Pole, and em-
braces every degree of longitude. This is a very considerable
portion of the surface of the globe, and it is one that has been con-
sidered to be for the most part inaccessible to man. I will therefore
ask you to accompany the scientific explorer no further than to the
threshold of the scenes of his labours, that you may see how soon
and how urgently he is called upon to study some of those hitherto
unsolved Antarctic problems that he will there encounter.

In latitude 60° S. an open ocean girdles the globe without break
of continuity. Proceeding southwards in it, probably before reach-
ing the Artarctic circle, he encounters the floating ice fields which
form a circumpolar girdle, known as "the Pack," approximately con-
centric with the oceanic, interrupted in one meridian only, that
south of Cape Horn, by the northern prolongation of Graham's
Land. Pursuing his southward course in search of seas or lands
beyond, after the novelty of his position in the pack has worn off, he

asks where and how the components of these great fields of ice had their origin, how they arrived at and maintain their present position, what are their rate of progress and courses, and what their influence on the surrounding atmosphere and ocean. I believe I am right in thinking that to none of these questions can a fuller answer be given than that they originated over extensive areas of open water in a higher latitude than they now occupy, that they are formed of frozen ocean water and snow, and that winds and currents have brought them to where we now find them. But of the position of the southern open waters, with the exception of the comparatively small area east of Victoria Land,* we know nothing, nor do we know anything of the relative amount of snow and ice of which they are composed, or of their age, or of the winds and currents that have carried them to a lower latitude.

The other great glacial feature of the Antarctic area is "the Barrier," which Ross traced for 300 miles, in the 78th and 79th degrees of S. latitude, maintaining throughout the character of an inaccessible precipitous ice-cliff (the sea-front of a gigantic glacier) of 150—200 feet in height. This stupendous glacier is no doubt one parent of the huge table-topped ice-islands that infest the higher latitudes of the Southern Ocean; but, as in the case of the pack ice, we do not know where the barrier has its origin, or anything further about it, than that it in great part rests on the bottom of a comparatively shallow ocean. It probably abuts upon land, possibly upon an Antarctic continent; but to prove this was impossible on the occasion of Ross's visit, for the height of the crow's nest above the surface of the sea was not sufficient to enable him to overlook the upper surface of the ice, nor do I see any other way of settling this important point except by the use of a captive balloon—an appliance with which I hope any future expedition to the Antarctic regions will be supplied. There were several occasions on which such an implement might have been advantageously used by Ross when he was coasting along the barrier; and there were more when it would have greatly facilitated his navigation in the Pack.

I have chosen the subject of the Antarctic ice as the theme for my acknowledgment of the honour you have done me in asking me to address this most important meeting, not only because it is one of the very first of the phenomena that demand the study of the explorer, but because it is the dominant feature in Antarctic navigation, where the ice is ever present, demanding, whether for being

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* I refer to the " pancake " ice, which in that area on several occasions formed with great rapidity around Ross's ships, lat. 76° to 78° S., in February, 1842, and which arrested their progress. Such ice, augmented by further freezing of the water and by snow, may be regarded as the genesis of fields that, when broken up by gales, are carried to the north and contribute to the circumpolar pack.

penetrated or evaded, all the commander's fortitude and skill and all his crew's endurance.

It may be expected that I should allude to those sections of Dr. Murray's summary that refer to the Antarctic fauna and flora. They are most important, for the South Polar Ocean swarms with animal and vegetable life. Large collections of these, taken both by the tow-net and by deep sea soundings, were made by Sir J. Ross, who was an ardent naturalist, and threw away no opportunity of observing and preserving; but unfortunately, with the exception of the *Diatomaceæ* (which were investigated by Ehrenberg), very few of the results of his labour in this direction have been published. A better fate, I trust, awaits the treasures that the hoped-for expedition will bring back, for so prolific is that ocean that the naturalist need never be idle, no, not even for one of the twenty-four hours of daylight during a whole Antarctic summer, and I look to the results of a comparison of the oceanic life of the Arctic and Antarctic regions as the heralding of an epoch in the history of biology.

Remarks by Dr. G. Neumayer.

With great pleasure I accepted the invitation to attend a discussion meeting on the importance, for the advancement of every branch of science, of a scientific exploration of the Antarctic region. Regardless of the season and my advanced age, I hastened here to speak in the presence of so high a forum as the Royal Society of London, on the necessity of despatching as soon as possible an expedition towards the South Pole—an expedition cannot be dispensed with if we seriously desire the advancement of nearly every branch of human knowledge. It is fifty-five years ago since one of the greatest Arctic and Antarctic explorers ceased his work, so exceedingly well executed, in the Antarctic regions, and, since that time, it has never been taken up in any way comparable with that glorious scientific and nautical spirit manifested by Sir James Clark Ross. It is in view of this fact that we all look to the British nation as the one destined to carry on the exploration of the South Polar regions, and to assist this object as much as lay in my power, and to do homage to the memory of Ross, I could not fail to appear at this meeting.

It is indeed a matter of great interest to examine the reason why so long a time has been allowed to elapse since the first great successes in the middle of this century. Undoubtedly political questions have interfered in an unusual manner so to retard progress in Antarctic inquiry, but it is not that alone; the cause is mainly that the thorough understanding of the importance of Antarctic research requires an unusual amount of knowledge, and not in one branch of science only, but in the whole complex of natural philosophy and

natural science. The positive advantages to be derived from a renewal of Antarctic exploration appear but few at first glance, and can only be detected by a far-sighted man. The Royal Geographical Society has taken up the agitation for that renewal. The Sixth International Geographical Congress, two and a half years ago, devoted its influential exertions to the recommendation of it to the Government, not only of this country, but, indeed, of all civilised countries. But now that the Royal Society has taken the matter up (an example which will be followed, I am convinced, by other academies of science), final success may be looked upon as matter of certainty.

However much as has been done already to urge the importance of the scientific investigation of the South Polar regions, from as many points of view as there are branches of science cultivated by mankind, I consider it my duty, as a representative of geophysical science, to add all the arguments in my power to those brought forward on the part of geologists, zoologists, botanists, and others.

A gravitation survey in connection with a thorough geographical exploration of the Antarctic, is one of the most urgent requirements of the science of our earth. There are no measurements of the gravitation constant within the Antarctic region, indeed they are very scarce in the southern hemisphere south of 30° south latitude. Such measurements are so closely connected with the theory of the figure of our earth, that it may well be asked how it can be considered possible to achieve any advance in this respect, to arrive at any conclusive results in this all-important fundamental matter, without observations within the Antarctic region. It is impossible to foretell what effect an exact gravitation survey in that region might exert upon our views with regard to all physical and terrestrial constants, which depend on the radius of our earth. Apart from that consideration, we may hope for another important enlargement of our knowledge bearing upon the connection between terrestrial magnetism and gravity. Gravity observations have been so much simplified of late by von Sterneck's ingenious apparatus, that there is no serious difficulty in so multiplying gravity determinations within the Antarctic region, that we shall be quite justified in speaking of a "gravitation survey." The all-important question of the distribution of land within the South Polar region is closely connected with it, and our leading authority on geodesy, Professor Helmert, justly lays great stress upon the observation of the force of gravity south of 60° south latitude. He induced the International Geodetic Permanent Commission to express it as their conviction that a gravitation survey within that region would be of the greatest benefit for higher geodetic theory (October, 1895).

I have already alluded to the probable connection between gravity

and terrestrial magnetism. But apart from that, a magnetic survey
of the Antarctic region is of the greatest importance from other
points of view. As, since the time of Ross, no fresh observations on
the values of the magnetic elements have been made, we are entirely
ignorant of the values of the secular variation south of 50° latitude,
data so much needed for the construction of reliable magnetic charts.
Of the situation of the southern magnetic pole, and of its motion
during the last fifty years, we are equally ignorant. The situation
of the southern extremity of the earth's magnetic axis and its motion
throughout half a century are extremely important according to
Gauss's theoretical deductions. All computations, however intri-
cate, must prove incomplete, and inadequately reward the immense
amount of labour bestowed upon them, unless reliable values of the
magnetic elements are first obtained, for which purpose a reduc-
tion to a certain epoch by means of secular changes is indispensable.
But not merely for theoretical purposes is such value to be put upon
the knowledge of the magnetic character in the Antarctic, but also
for the construction of reliable magnetic charts for use in naviga-
tion.   Certainly the studies of Carlheim-Gyllenskjöld and Van
Bemmelen deserve great credit from a theoretical point of view,
but they also cannot be carried to perfection unless we have a sound
knowledge of the magnetic character of the South Polar region.—
at least as sound as that of the North Polar region. In the time
of Ross, the magnetic Observatory at Hobart served as a safe
basis for observations on terrestrial magnetism, and now the excel-
lent Observatory at Melbourne may be turned to a similar account.

    Well as the mathematical theory of terrestrial magnetism has
been developed, of the physical theory of that mysterious natural
force, we are as yet in perfect ignorance. This defect is certainly
to some considerable degree caused by the deficiency of our know-
ledge in higher latitudes. It looks as if the magnetic character of
the South Polar region were such as would afford every facility for
a sound investigation when brought into comparison with the
magnetic conditions of the North Polar region. A glance at the
map shows how entirely different is the distribution of the magnetic
force (action) in the two polar regions. In the south there is to be
noticed the interesting fact that the two foci of total intensity are
both situated on the side towards the south of the Australian
continent, and nearly on the same meridian.

    The magnetic action which makes itself manifest by magnetic
storms or disturbances reaches its highest degree likewise south of
the Australian continent, whereas to the south of South America
these disturbances become very scarce and of a character in point
of magnitude similar to those of the temperate zones. This was
most strikingly proved by the observations in Orange Bay and

South Georgia during the period of International Observations in 1882–83. Of course, the magnetic South Pole and the situation of the foci above mentioned are in close connection with these facts, but the reason for this distribution remains unexplained. In agreement with this is the relative frequency of the South Polar lights within one and the same epoch. The map here shows the results of a discussion of all observations on southern lights, and, as may be perceived at a glance, the conformity with the fact just mentioned with regard to the region of maximum disturbance is striking. It may be mentioned, also, that a discussion of the magnetic state of our earth for the epoch 1885 has yielded a curious fact (perhaps merely a coincidence), namely, that Dr. Schmidt, of Gotha, has calculated that part of the magnetic action of our earth which lies outside it, above the earth's atmosphere probably, and has arrived at the conclusion that this part amounts to about 1/50th part of the entire potential. The curves which he constructed, based on this calculation, show likewise a close coincidence with the frequency of the southern lights. This requires to be further investigated, for it is perhaps a mere coincidence. The question of atmospheric electricity, yet under the shadow of a hypothesis of more or less probability, may yield, in connection with the matters touched upon, some results more definite than science has hitherto been able to divine. Under all these circumstances we perceive that there are a number of problems yet to be solved, which must stimulate the scientific world to enter upon a close and conscientious examination of a region still enveloped in total darkness.

The necessity of climatological researches within the Antarctic region has already been so much urged that it does not appear necessary for me to enter upon the matter. That it must remain impossible to arrive at exact climatological constants, so long as we do not know the winter temperature in that large area round the South Pole and bounded by the South Polar circle, is evident to every one conversant with the subject and does not require any further illustration.

With regard to atmospheric pressure there is another curious fact which requires investigation, only to be carried on by observations within the Antarctic. The excess of barometric pressure in 35° latitude over that in 60° latitude amounts in the northern hemisphere to 19·0 mm. and in the southern hemisphere to only 3·7 mm. Here is another problem worthy of examination.

With the recital of these few facts, which suffice to prompt us to institute a vigorous examination of the South Polar regions the series is far from being exhausted. There is the question of the geoid-deformation, the phenomena of the tides, the structure of the ice and its drifting, especially the interesting fact of the appearance

of ice in lower southern latitudes, where it has never been observed
since observations thereon are recorded.

The resolution of the Sixth International Congress of Geographers,
assembled here in August, 1895, according to which the present
century ought not to be allowed to expire without the unveiling of the
mysteries of the South Polar regions, ought to be carried into execu-
tion by sending out an expedition to that end. All scientific institu-
tions and societies have a well founded interest that such expedition
should take place without further delay.

Remarks by Sir Clements Markham.

I need scarcely say how fully I concur in every word that has
fallen from Dr. Murray on the subject of the scientific results, and
more especially of the geographical results of an Antarctic Expe-
dition. It is quite sufficient to point out the vast extent of the
unknown area, and that no area of like extent on the surface of the
earth ever failed to yield results of practical as well as of purely
scientific value by its exploration.

But there is much more to be said in the present instance, because
the little that we do know of the Antarctic regions points unmis-
takably to the very great importance and interest of the results
that are certain to attend further research. The ice barrier dis-
covered by Sir James Ross is known to be the source of the immense
ice islands of the Southern Polar Sea. But it has only been seen for
a distance of 300 miles. It requires far more complete examination
before any approach to an adequate knowledge can be obtained
respecting the extent and nature of the supposed ice cap in its rear.

We know that the Southern Continent is a region of actual
volcanic activity, but the extent, nature, and effects of that activity
remain to be ascertained. On the Antarctic Circle land has been
sighted at numerous points, but it is unknown whether what has
been seen indicates small islands or a continuous coast line.

Dr. Murray has pointed out that the whole Southern Continent is
certainly not bounded by such an ice wall as was seen by Sir James
Ross, and is not covered by an ice cap. But the extent of the ice
cap and of the uncovered land is unknown. We are ignorant of the
distribution of land and sea, and of ice and water in summer, and of
the causes which influence such distribution.

The investigation of each one of these points, and of many others,
will lead to further discoveries as yet undreamt of, which must needs
be of the deepest interest to geographers. There are eminent
scientific men present who will no doubt refer to the results of
exploration in other branches of science. Combined together they
make the exploration of the Antarctic regions the greatest and most
important work that remains to be achieved by this generation.

Remarks by Dr. Alexander Buchan.

Dr. Alexander Buchan stated that his remarks would have exclusive reference to the first two paragraphs of Dr. Murray's address, under the heading of "The Atmosphere," but more immediately to the relation between mean atmospheric pressure and prevailing winds. He supposed he had been asked to speak on this occasion from the extensive and minute knowledge of the subject he had necessarily acquired in the preparation of the reports on atmospheric and oceanic circulation which were published as two of the reports of the scientific results of the voyage of H.M.S. "Challenger."

The former of these reports on atmospheric circulation, is accompanied by twenty-six maps showing, by isobars for each month and the year, the mean pressure of the atmosphere and by arrows the prevailing winds of the globe, on hypsobathymetric maps, or maps showing by shadings the height of the land and the depth of the sea; first, on Gall's projection, and second, on north circumpolar maps on equal surface projection. The isobars are drawn from mean pressures calculated for 1366 places, and the winds from even a larger number of places distributed as well as possible over the globe. It may also be noted that the figures showing the averages of pressure and prevailing winds are published with the report, accompaniments to maps of mean atmospheric pressure and prevailing winds of the globe not yet attempted by any other writer who has published such maps.

This, then, is the work undertaken and published in these reports, which occupied seven years in preparing as time could be spared from official duties. The result of the charting of the pressure and prevailing winds is this: Stand with your back to the wind, then the centre of lowest pressure that causes the wind is to the left in the northern hemisphere and to the right hand in the southern hemisphere, a relation well known as Buys Ballot's law. In charting the 1366 pressures and the relative prevailing winds no exception was found in any of the two hemispheres. This is one of the broadest generalisations science can point to, so far as regards what takes place in the free atmosphere.

Some years ago a theory of atmospheric circulation was published by the late Professor Ferrel, which, as it is not accordant with the broad results arrived at in the report on atmospheric circulation in the "Challenger" reports, calls for serious consideration on account of its bearing on any attempt proposed to be undertaken for the exploration of the Antarctic regions.

One of the more recent expositors of this theory is Professor Davis, of Harvard College, who, in his 'Elementary Meteorology,' Boston, U.S.A., 1894, gives an admirable exposition of the results

now arrived at by the various workers in meteorology, and of the
opinions and theories promulgated by different meteorologists in
different departments of the science. The book is largely used in
secondary schools and colleges of the United States, and the views
there stated are widely held in America and are spreading into other
countries.

. The following extracts from Davis's book fairly represent these
views :—

"The surface winds of the temperate latitudes, and the high level
currents above them, sidling swiftly along on their steep poleward
gradients, must all be considered together. They combine to form a
vast aerial vortex or eddy around the pole. In the northern hemi-
sphere this great eddy is much interrupted by continental high
pressure in winter or low pressure in summer, and by obstruction
from mountain ranges, as well as by irregular disturbances of the
general circulation in the form of storms" (p. 110).

. Now the facts of observation do not support the theory of the
existence at any season of the year, of a low barometric pressure, or
an eddy of winds, round or in the neighbouring regions of the North
Pole. Observations show us no prevailing winds blowing home-
wards to the region of the North Pole at any time of the year.
No low barometric pressure occupies the immediate polar region in
any month; but instead the opposite holds good for the four months
from April to July. In April and May the mean atmospheric pres-
sure is higher in the region of the Pole than it is anywhere in the
northern hemisphere north of 43° lat. N.; and in June and July
also higher than it is anywhere north of 55° lat. N. Now the
higher pressure in these four months necessitates the existence of
upper currents in order to maintain this high pressure about the
North Pole. These upper currents towards the pole are exactly
opposed to the requirements of the theory that the upper currents
in the region of the Pole must necessarily blow not towards but
from the Pole.

The actual centre, in this hemisphere, north of the tropic towards
which the winds on or near the surface of the earth blow, is not
the North Pole; but in the winter months the low barometric de-
pressions in the north of the Atlantic and Pacific respectively, and
in the summer months the low barometric depressions in the Eura-
sian and North American continents; and the sources out of which
the prevailing winds blow in the winter months, the high-pressure
regions in Siberia and North America; and in the summer months
the high-pressure regions lying northward of these continents,
which, as already explained, are virtually the polar region itself.
These are the facts in all regions where the winds, according to
the theory, become winds blowing over the earth's surface.

As regards the southern hemisphere, Professor Davis states that : "In the southern hemisphere the circumpolar eddy is much more symmetrically developed." Again, "the high pressure that should result from the low polar temperatures is therefore reversed into low pressure by the excessive equatorward centrifugal force of the great circumpolar whirl; and the air thus held away from the polar regions is seen in the tropical belts of high pressure" (pp. 110, 111).

The meaning of this is that the remarkable low-pressure region of the southern hemisphere is continued southward to the South Pole itself, the pressure diminishing all the way; and that in the region of the South Pole the air currents poured thitherwards along the surface of the earth ascend and thence proceed northwards as upper currents of such enormous intensity and volume that they pile up in the tropical region of the southern hemisphere a mean sea-level atmospheric pressure about an inch and a half higher than the sea-level pressure near the South Pole whence it has started.

Now, to bring the matter to the business which this meeting of the Royal Society has in hand—if this theory were true and supported by the facts of observation—it is plain that no meteorologist could signify his approval of any scheme that could be proposed for exploring the Antarctic regions, it being obvious that these strong west-north-westerly winds, if they blow vortically round and in upon the pole, heavily laden, as they necessarily would be, with the aqueous vapour they have licked up from the Southern Ocean, would overspread Antarctica with a climate of all but continuous rains, sleet, and snow which no explorer, however intrepid and enthusiastic, could possibly face.

But is this the state of things? Let it be at once conceded that, as far south as about 55° lat. S., the prevailing winds and the steadily diminishing mean pressures on advancing southward, fairly well support the theory. South of this, however, southerly and south-easterly winds begin to increase in frequency, until from 60° lat. S. into higher latitudes, they become the prevailing winds. This is abundantly shown from the winds charted on the maps of the "Challenger" Report, as well as from the unanimous experience of all that have navigated this region from Ross to the present time. Thus the poleward blowing winds from west-north-west in these summer months stop short, distant at least 30° of latitude from the South Pole.

These prevailing south-easterly winds necessarily imply, as has been shown in the analogous case of the North Pole, the existence of a more or less pronounced anticyclone overspreading Antarctica; which in its turn necessarily implies the existence of upper currents from the northward, blowing towards and in upon the polar region to make good the drain caused by the surface out-blowing south-

easterly winds. It may, therefore, be concluded that both the surface winds and the upper aerial currents are diametrically opposed to the requirements of this theory.

What is now urgently called for is a well-equipped Antarctic Expedition to make observations which will enable meteorologists to settle definitely the distribution of atmospheric pressure and the prevailing winds of this great region. Were this done, the position in the Southern Ocean of the great ring of lowest pressure that encircles the globe could be mapped out; and since it is towards such a low-pressure ring that the wind-driven surface currents of the ocean flow, a contribution would thereby be made to oceanography, of an importance that cannot be overestimated, particularly as regards the great question of oceanic circulation.

Remarks by Sir A. Geikie.

Hardly anything is yet known of the geology of the Antarctic regions. By far the most important contributions to our knowledge of the subject were made by the expedition under Sir James Ross. But as he was unable to winter with his ships in the higher latitudes, and could only here and there with difficulty effect a landing on the coast, most of the geological information brought home by him was gathered at a greater or less distance from the land with the aid of the telescope. Within the last few years several sealing vessels have brought home some additional scraps of intelligence which only increase the desire for fuller knowledge.

As regards the land, merely its edges have here and there been seen. Whether it is one great continent, or a succession of islands and archipelagos, may possibly never be ascertained. We know that in Victoria Land it terminates in a magnificent mountain range with peaks from 10,000 to 15,000 feet high; but that elsewhere it is probably comparatively low, shedding its ice-cap in one vast sheet into the sea.

The rocks that constitute the land are still practically unknown. The dredgings of the "Challenger" Expedition brought up pieces of granite, gneiss, and other continental rocks, and detritus of these materials was observed to increase on the sea-floor southwards in the direction of the Antarctic land. More recently several sealing vessels have brought home from the islets of Graham Land, to the south of the South Shetlands, pieces of different varieties of granite, together with some volcanic rocks and fossiliferous limestones. So far as these rocks have been studied, they do not appear to differ from similar rocks all over the globe. The granites have been found by Mr. Teall to be just such masses as might have come from any old mountain-group in Europe or America.

Among the specimens sent to me by Captain Robertson, of the

" Active," from Joinville and Dundee Islands, which form the north-eastern termination of Graham Land, there was one piece of reddish jasper which at once attracted my attention from its resemblance to the "radiolarian cherts" now found to be so widely distributed among the older Palæozoic rocks, both in the Old World and in the New. On closer examination, this first impression was confirmed; and a subsequent microscopic study of thin slices of the stone by Dr. Hinde proved the undoubted presence of abundant radiolaria. The specimen was a loose pebble picked up on the beach of Joinville Island. We have no means of telling where it came from or what is its geological age. But its close resemblance to the radiolarian cherts, so persistent in the Lower Silurian formations of the United Kingdom, raises the question whether there are not present in the Antarctic regions rocks of older Palæozoic age.

It would be of the utmost interest to discover such rocks *in situ*, and to ascertain how far their fossils agree with those found in deposits of similar antiquity in lower latitudes; or whether, as far back as early Palæozoic time, any difference in climate had begun to show itself between the polar and other regions of the earth's surface.

Among the specimens brought home by Dr. Donald and Captain Larsen from Seymour Island in the same district are a few containing some half dozen species of fossil shells, which have been named and described by Messrs. Sharman and Newton, who suggest that they point to the existence of Lower Tertiary rocks, one of the organisms resembling a form found in the older Tertiary formations of Patagonia. Large, well-developed shells of *Cucullæa* and *Cytherea* undoubtedly indicate the former existence of a far milder climate in these Antarctic seas than now prevails.

If a chance landing for a few hours on a bare islet could give us these interesting glimpses into the geological past of the south polar regions, what would not be gained by a more leisurely and well planned expedition ?

But, perhaps, the geological domain that would be most sure to gain largely from such exploration would be that which embraces the wide and fascinating field of volcanic action. In the splendid harvest of results brought home by Sir James Ross, one of the most thrilling features was the discovery of a volcano rising amid the universal snows to a height of more than 12,000 feet, and actively discharging "flame and smoke," while other lofty cones near it indicated that they too had once been in vigorous eruption. Ross landed on one or two islands near that coast and brought away some pieces of volcanic rocks.*

If we glance at a terrestrial globe we can readily see that the

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* His collections are in the British Museum, but they have never been petrographically studied.

volcanic ring or "circle of fire," which nearly surrounds the vast
basin of the Pacific Ocean, stretches southwards into New Zealand.
The few observations that have been made in the scattered islands
further south show that the Auckland, Campbell, and Macquarrie
groups consist of, or at least include, materials of volcanic origin.
Still further south, along the same general line, Mr. Borchgrevink
has recently (1894-95) made known the extension of Ross's volcanic
region of Mount Erebus northwards to Cape Adare, the northern
promontory of Victoria Land. He noticed there the apparent
intercalation of lava and ice, while bare snowless peaks seemed still
further to point to the continued activity of the volcanic fires.
Some specimens, brought by Captain Jenssen, from Possession
Island were found by Mr. Teall to be highly vesicular hornblende-
basalt; while one from Cape Adare was a nepheline-tephrite.
This region is probably one of the most interesting volcanic tracts
on the face of the globe. Yet we can hardly be said to know more
of it than its mere existence. The deeply interesting problems
which it suggests cannot be worked out by transitory voyagers.
They must be attacked by observers stationed on the spot. Ross
thought that a winter station might be established near the foot of
Mount Erebus, and that the interior could easily be traversed from
there to the magnetic pole.

But it is not merely in Victoria Land that Antarctic volcanoes
may be studied. Looking again at the globe, we observe that the
American volcanic band is prolonged in a north and south line
down the western side of the southern continent. That it has been
continued into the chain of the South Shetlands and Graham Land
is proved by the occurrence there of old sheets of basalt, rising in
terraces over each other, sometimes to a height of more than
7,000 feet above the sea. These denuded lavas may be as old as
those of our Western Isles, Faroe, Iceland, and Greenland. But
that volcanic activity is not extinct there has recently been found
by Captain Larsen, who came upon a group of small volcanoes form-
ing islets along the eastern coast line of Graham Land. It is
tantalising to know no more about them.

Another geological field where much fresh and important infor-
mation might be obtained by Antarctic exploration is that of ice
and ice action. Our northern hemisphere was once enveloped in
snow and ice, yet although for more than half a century geologists
have been studying the traces of the operations of this ice covering,
they are still far from having cleared up all the difficulties of the
study. The Antarctic ice-cap is the largest in the world. Its
behaviour could probably be watched along many parts of its
margin, and this research would doubtless afford great help in the
interpretation of the glaciation of the northern hemisphere.

To sum up :—Geologists would hail the organisation and despatch of an Antarctic Expedition in the confident assurance that it could not fail greatly to advance the interests of their science. Among the questions which it would help to elucidate, mention may be made of the following :—

The nature of the rocks forming the land of the Antarctic regions and how far these rocks contain evidence bearing on the history of terrestrial climates.

The extent to which the known fossiliferous formations of our globe can be traced towards the poles; the gaps which may occur between those formations and the light which their study may be able to throw on the evolution of terrestrial topography.

The history of volcanic action in the past and the conditions under which it is continued now in the polar regions; whether in high latitudes volcanism, either in its internal magmas or superficial eruptions, manifests peculiarities not observable nearer to the equator; what is the nature of the volcanic products now ejected at the surface; whether a definite sequence can be established from the eruptions of still active volcanoes back into those of earlier geological periods; and whether among the older sheets leaf-beds or other intercalations may be traceable, indicating the prolongation of a well developed terrestrial flora towards the South Pole.

The influence of the Antarctic climate upon the rocks exposed to its action; the effects of contact with ice and snow upon streams of lava; the result of the seaward creep of the ice-cap in regard to any lava sheets intercalated in the ice.

The physics of Antarctic ice in regard to the history of the Ice Age in Northern Europe and America.

Remarks by Professor D'Arcy W. Thompson.

The exploration of the Antarctic gives promise of gains to zoological knowledge that are in no degree less, in my opinion, than in the case of the physical sciences. The shore-fauna of the Antarctic we know only by a few scanty collections made upon the islands, especially upon Kerguelen Island; and the fauna of the deep sea is only represented by the produce of eight hauls of the " Challenger's " dredge. These few dredgings gave evidence of peculiarly abundant life, indeed they were said, by common consent of the naturalists of the " Challenger," to be the richest dredgings in all her voyage : and they were as remarkable for the diversity as for the abundance of the animals they procured. We earnestly desire, and the progress of zoological science needs, further exploration of the deep-sea fauna in all the oceans. Our knowledge of the fauna of the deep sea is only begun; it is known much as the fauna of the

shore was known a hundred years ago, and we want to know more of the undiscovered forms that are peculiar to it, and more of the struc-ture and affinities of those already discovered. But apart from innumerable facts appertaining to systematic or morphological zoology, such as every ocean has yet to yield to us, there are certain great problems of geographical distribution to which the Antarctic is peculiarly likely to give a clue. Lying open to, as it were at the confluence of, all the great oceans, its fauna may co-ordinate and explain many things in the divergent faunas of the Indian Ocean, the Atlantic, and the Pacific. One such problem Dr. Murray has touched upon in his hypothesis of "bipolar distribution," that is to say, of a general similarity and in many cases of actual identity between the animals of the Arctic and Antarctic Seas. This theory has been proclaimed before by others, by Théel, for instance, and by Pfeffer; but others have contested and denied it; for instance, Ortmann in regard to the crustacea, and Chun in regard to pelagic organisms. We could not have a better illustration of the poverty of our knowledge than the circumstance that so broad and clear and simple an issue as the existence or non-existence of a close relation be-tween the Arctic and Antarctic faunas should still be open to dispute.

For my part I think that the bipolar hypothesis is not proven, and I am inclined to think it is untrue. I believe with Ortmann that in the Decapod crustacea at least one form is common to the Arctic and Antarctic Seas; and with Chun and others that there is evidence that similar pelagic forms occurring in the north and south, though not in the surface waters of the tropical seas, are in these latter continued across the torrid zone in the deeper and cooler levels of the sea. I do not think that any single fish, or any Decapod or Isopod, or any certain one out of a large fauna of Amphipods known from the Antarctic Ocean, is also known from the Arctic and adjacent seas. It seems to me, however, that we have some good evidence of very curious similarities between the marine fauna of the Antarctic and that of the N. Pacific in the neighbourhood of Japan; and it may be that this is to be in part explained by the existence of a line of communication along the Western American coast, in waters singularly cold for the latitude under which they lie. We know how, in this way, such conspicuous forms as the genus *Serolis*, the Penguins, the Sea-elephant, the Sea-lions, and the Fur-seals, I might add the giant Sea-weed *Macrocystis* seem to creep up the American shore, from what was probably their Antarctic home, to Chili, to the Galapagos, or even to North Pacific and to Japan. But these are illustrations merely of the zoological problems that Ant-arctic exploration may solve. If the bipolar hypothesis be broken down, it will only give place to other hypotheses as interesting as itself. New facts will give rise to new hypotheses, for further facts to verify

or to disprove, and the Antarctic holds for us innumerable problems of which we can foresee neither statement nor solution, as well as the solution of those that we can already in some measure foresee.

"On the Zoological Evidence for the Connection of Lake Tanganyika with the Sea." By J. E. S. MOORE, A.R.C.S. Communicated by Professor LANKESTER, F.R.S. Received January 12,—Read January 27, 1898.

(From the Huxley Research Laboratory, Royal College of Science, London.)

Before 1896, when I had the opportunity of studying the fauna of Lake Tanganyika on the spot, it was known that there existed in the so-called Sea of Ujiji, one animal, the affinities of which are undoubtedly marine. This was the medusa *Limnocnida*, which Dr. Boehm saw as he crossed the lake in 1883.

It was known further that the jelly-fish was associated in Tanganyika with a number of strange molluscan forms, for the empty shells of what appeared to be some six entirely new genera of gasteropods, had been brought home by Captain Speke, Joseph Thomson, and Mr. Hore. As the animals contained in these shells have not hitherto been known, their classification by the conchologists with existing fresh-water types has always appeared extremely doubtful, and from the first Mr. Edgar Smith, who described the greater number of these forms, has held the opinion that they might eventually turn out to have the same oceanic characters as the jelly-fish.

It was therefore one of the objects of my recent expedition to obtain material for the complete determination of these molluscous types, and especially to ascertain if there were any other marine organisms in the lake. The results of this attempt have been to show :—

I. That to the six genera of quasi-marine gasteropods, the shells of which were known, viz., *Typhobia*, *Nassopsis*, *Limnotrochus*, *Syrnolopsis*, the so-called *Lithoglyphus*, and *Paramelania*, there are now to be added at least two, entirely new generic forms, for which I propose the names* *Bathanalia* and *Bythoceras* (figs. 1 and 2). We have therefore now representing the quasi-marine molluscs of Tanganyika eight genera of gasteropods, and to these should probably be added among the Lamellibranchiata the so-called *Unio Burtoni* and one of the Tanganyika Spathas.†

* Diagnoses of these new genera will be found in papers now in the hands of the Editor of the ‘Quart. Journ. Micr. Sci.’

† Complete accounts of the anatomy of all these Halolimnic genera will shortly appear in the ‘Quart. Journ. Micr. Sci.’

II. That among the invertebrate population of Tanganyika there are a large number of widely separated animal types, all of which

FIG. 1.—Shell of *Bythoceras iridescens* obtained living near Sumbu, Tanganyika, at a depth of 680 ft.

FIG. 2.—Shell of *Bathanalia Howesii*, obtained near Mlerocs, Tanganyika, at a depth of 950 ft.

possess the same quasi-marine affinities. Thus I found that among the Crustacea there are two forms of prawns and one deep-water crab. Among the Hydrozoa the already known medusa *Limnocnida*. Among Porifera one deep-water sponge; and lastly there are several forms of *Peridinea* and *Condylostoma* among the Protozoa. A large proportion of these organisms are exceedingly peculiar; but others such as the two prawns, the deep-water crabs and sponge, and pos-

sibly the pelagic Protozoa, are much more nearly related to the similar marine organisms which have repeatedly contaminated the fresh waters of the world elsewhere. It should, however, be clearly understood that even these apparently more normal types have not been found in Nyassa, Shirwa, or Kela, nor have they been recorded from any of the Great Lakes further north. Therefore, although they are less peculiar than their associates in Tanganyika, they probably belong to the same quasi-marine, or what I shall in future call the *Halolimnic* group.

The results of a systematic survey which was made of the geographical and bathymetric distribution of the aquatic molluscs throughout the wide area over which the expedition had to pass have demonstrated in the most conclusive manner the complete duality of the Tanganyika fauna as a whole.* In Nyassa, Shirwa, Kela, and several minor lakes, taken together, all of which I visited and dredged, there have been found the following molluscous types:— *Unio, Spatha, Corbicula, Iridina, Limnœa, Isodora, Physopsis, Planorbis, Ancylus, Ampullaria, Lanistes, Vivipara, Cleopatra, Bithynia,* and *Melania.*

Not all of these fifteen genera which are now found living in Nyassa are present in the smaller lakes. In the Shirwa they are reduced to five, and in Kela they are only three. The full Nyassan series has, however, been recorded from the Victoria Nyanza, and in this more northern group of lakes there is again seen the curious reduction in the number of genera as we pass from the greater lakes to the less. From these facts of distribution it is apparent that the genera of molluscs, which occur in the African fresh waters, are very constant over an immense area of ground. There can indeed be little doubt that the genera found in Nyassa characterise and constitute the type of the truly African fresh-water fauna as a whole.

The fauna of Tanganyika appears therefore to form a striking contrast to this rule of uniformity in type which characterises the fauna of all the other lakes. Such divergence is, however, in one sense more illusory than real. All the Nyassa or Victoria Nyanza genera are found living in Tanganyika, and the fauna of this lake does not differ from the faunas of the others in kind or as a whole. It differs from them merely in there being here added to the normal series a number of molluscs which are not found elsewhere. To this superadded list, however, there attaches a unique interest, as it is entirely composed of those ten genera of gasteropods and lamellibranchs which were instanced as possessing the same marine appearance as the jelly-fish and prawns.

The strange geographical isolation of the halolimnic molluscs which

* The full details of my observations on the distribution of the Halolimnic molluscs are now in the hands of the Editor of the ' Quart. Journ. Micr. Sci.'

these facts disclose is also true and characteristic of all the other halolimnic animals I have named. It is thus rendered evident that the entire halolimnic fauna as it exists in Tanganyika now is something completely distinct from and superadded to the normal African lake fauna as a whole. This fact is of the utmost import when we attempt to ascertain from what source the halolimnic animals have sprung.

The isolation of these animals shows conclusively that they cannot have arisen, so to speak, *de novo* in Tanganyika through the effect of the conditions under which they live, for if this were so there would have arisen similar halolimnic animals under the apparently similar conditions which exist elsewhere. For the same reason they cannot be regarded as the surviving representatives of an older fresh-water stock, since were this the case we should have to believe that this old stock had been destroyed in every African lake but one. Nyassa, moreover, appears to have been a fresh-water lake longer than Tanganyika, yet in the Post-pleistocene deposits which occur along its shores no halolimnic fossils have been found.

Now it is perhaps conceivable that prawns, which are active vigorous organisms, could by great exertions have made their way up the numerous falls along the single effluent of Tanganyika from the sea in recent times, for they have certainly thus entered many lakes already known. But with respect to the remaining halolimnic organisms, there is a singular feature common to them all which effectually precludes any possibility of this. All these animals are incapable of being directly associated with any *living* oceanic species. This fact alone demonstrates conclusively that the halolimnic fauna, wherever it came from, must be old. It has either had time to modify into its present condition from forms which are already known, or, what is more probable, it has more or less adhered to the characters of the older types from which it sprang. Delicate organisms, such as the Medusa, could not have found their way up the effluent as it now exists: it is barely conceivable that they can have been carried overland, while it is altogether out of the question to suppose that either of these processes could account for the presence of the halolimnic molluscs in the lake, as these are almost exclusively deep water forms. The genus *Typhobia* and the genus *Bathanalia* are generally beyond the hundred fathom line. *Limnotrochus* and *Syrnolopsis* are never found in less than 200 feet, and they occur up to 700 feet. The morphology of these molluscs is therefore of the first importance in determining the nature of the halolimnic group, for if the affinities of these organisms have been misinterpreted, and if in reality it can be shown that they have been derived from ancient oceanic types, they must have made their way into Tanganyika from the sea under widely different conditions from those which now exist; in fact, the

proof of their oceanic character will more or less necessitate the idea
that the Tanganyika region of to-day must have approximated in
character to an arm of the deep and open sea in ancient times.

During my late expedition I was able to obtain sufficient material
for the complete morphological investigation of all the halolimnic
molluscs, the shells only of which have hitherto been known, as well
as for the two new genera *Bathanalia* and *Bathoceras* represented in
figs. 1 and 2, and as I have worked over in detail a considerable number
of these forms I am now in a position to state definitely what they
really are. In 1857 S. P. Woodward, when describing the shells of
the so-called *Lithoglyphus* of Tanganyika, which had been obtained
by Speke, observed " the univalve . . . . so much resembles a
Nerita or *Calyptræa* that it would be taken for a sea shell if its
history were not so well authenticated," and similar reflections
were made by other observers when describing the shells more
recently obtained by Captain Hore.

But possibly owing to the weight then attached to Murchison's
geological speculations respecting the African interior, undoubtedly
to the fact that the Tanganyika jelly-fish was not then known, and
also because the fresh-water habitat of these molluscs was indisput-
able, the idea of their marine origin which was thus distinctly
before the minds of older zoologists subsequently became entirely
obscured. The shell of Typhobia was hesitatingly classed by
Smith in 1881, and more definitely by Fischer in 1887, with the
Melanidæ* as a subsection of that group. The shells of the Para-
melanias were regarded as nearly related to the same, while the
really unique so-called *Lithoglyphus* of Tanganyika was equally
misplaced. The mere fact of the jelly-fish being, as I ascertained,
associated with other marine organisms in Tanganyika would throw
suspicion on these purely conchological determinations, and the
actual anatomical character of the halolimnic molluscs entirely con-
firms this view. The Typhobias are utterly unlike any *Melania* the
anatomy of which is known. These gasteropods in the character of
their radulæ and their alimentary canals, in the presence of a crys-
talline style and an anterior stomachic cœcum, in the possession of a
well-developed posterior and anterior syphon, in the form of the
gills and osphradium, in the position of the anal, genital, and renal
apertures, as well as in the gross details of their reproductive
apparatus, most closely approximate to *Strombus* and *Pteroceras*.
The same inference may be gathered from the longi-commissurate
character of the nervous system, while in the absence of a right pallial
anastomosis, as well as in the form of the subintestinal ganglionic

* In 1881 Smith became acquainted with the operculum of *Typhobia*, which
seemed to confirm this opinion, but it is evident he doubted its correctness from
statements on the same page. ('Zool. Soc. Proc.,' 1881, p. 298.)

trunk, the Typhobias undoubtedly approximate to the Solaridæ and possibly to the Scalarids.   In fact, the structural *tout ensemble* of the Typhobias leaves little room for question that these gasteropods must be regarded as forms closely similar to a Pteroceras with a non-specialised foot.

What is true of the Typhobias is also true of the allied genus *Bathanalia*, except that this form is in some respects more primitive, and is certainly less specialised in its shell.   The so-called *Lithoglyphus zonatus*, *L. neritinoides*, and *L. rufofilosus* are seen at once, when anatomically examined, to have been perhaps even more completely misplaced than the Typhobias.   In the characters of their radulæ and alimentary canals they approximate to the *Planaxidæ*,* while in the possession of an anterior stomachic cœcum and style, they show undoubted affinity to some members of the Strombidæ.   In the character of their nervous system they are undoubtedly akin to the marine Zygoneurous *Cerithia* on the one hand and the longi-commissurate *Struthiolariidæ* on the other.   But the most remarkable anatomical feature which these forms possess is the existence in the female of an enormous epidermal invagination of the body wall beneath the eye (fig. 3), into which the embryos descend from the female genital aperture along a deep groove, and I have now complete evidence for regarding this groove, which is present in both sexes as truly homologous with the similar genital grooves possessed by the *Opisthobranchs*.   The affinities of the new genera *Bythoceras*, *Paramelania*, and *Nassopsis*, are much more difficult to determine, but there is no doubt that in the curious condition of their nerves and in the general features of their anatomy they are extremely primitive.   The whole nervous system of these forms, in the forwardly elongated character of the pedal ganglia and in the relation and characters of the cerebral and pleural ganglia and their connectives, actually approximate to that of a Cyclophorus.   In other respects it resembles that of *Triton*.

Lastly, the one entire *Limnotrochus* which I possess seems to be nearly akin to the Paramelanian group, but the anatomy of this form will require more fully working out by sections than has yet been done.

Thus, although I am not yet able to give a complete statement of the character of all the halolimnic molluscs known, enough anatomical work has now been done for this preliminary communication to indicate clearly what will be the entire result of the investigation. It has been seen that the theory of the marine origin of the

* It is remarkable that representatives of this family abound in the Indian Ocean and on the East African coast, the so-called *Lithoglyphus* of Tanganyika affording one among many instances of similarity between the molluscan fauna of Tanganyika and that of the Indian Ocean.
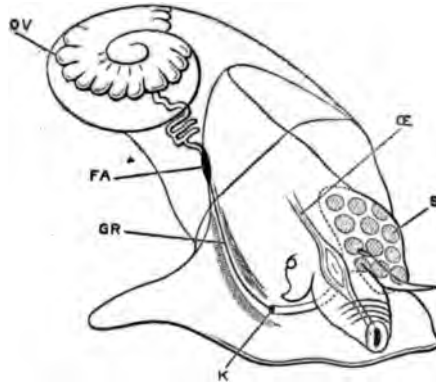
Fɪɢ. 3.—Semidiagrammatic representation of the reproductive apparatus in the female of the so-called *Lithoglyphus rufofilosus.* OV, Female genital gland. FA, Opening of oviduct. GR, Genital groove. K, Opening of brood pouch. S, Ova contained in brood pouch. Œ, Œsophagus.

halolimnic fauna of Lake Tanganyika is entirely supported both by the facts of distribution and by those of the morphology of the individual halolimnic forms. Like the medusa, the halolimnic gasteropods combine the characters of several *modern* marine types, and so they cannot by any possibility be regarded as the forerunners of the modern fresh-water stocks.* Consequently, the only way in which their existence in Tanganyika can be accounted for is through the supposition that this region was, as Thomson supposed, at some time in open connexion either on the east, west, or north, with a deep arm of the sea.

Such a conception is, however, in the most uncompromising conflict with the views respecting the permanence of the African terrestrial conditions which were advanced by Sir Roderic Murchison in 1852,† and which have been more recently and so ably advocated by Dr. Gregory in 1896.‡ Nevertheless, the theory of the marine

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* It is certain from their anatomical characters that some of the halolimnic molluscs (the Typhobias) originated from marine ancestors later than Cretaceous times, for they possess the characters of genera such as *Strombus* and *Pteroceras,* *i.e.,* genera that are Post-cretaceous and marine. Of the latter of these genera M. Fischer indeed remarks : " L'existence de ce genre à l'état fossile paraît douteuse " (' Manuel de Conchyliologie et Paléontologie Conchyliologique,' p. 671).

† ' Roy. Geogr. Soc. Journ.,' vol. 24, 1864, pp. clxxv—clxxviii.

‡ In his work, ' The Great Rift Valley,' p. 214, Gregory restates the geological position as follows :—" That part of Murchison's theory, which affirms that Central Africa has never been below the level of the sea, is still in harmony with the known facts, for no deposits of marine origin have as yet been found in the interior."

origin of the halolimnic fauna is now supported by an accumulating mass of the strongest kind of zoological evidence it is possible to obtain, while the above geological speculations to which it is diametrically opposed are based at best merely on the continued absence of all definite information respecting the past geological history of the " far interior" of any sort or kind.

OBITUARY NOTICES OF FELLOWS DECEASED.

BENJAMIN APTHORP GOULD was born at Boston, on September 27, 1824. He entered the Boston Latin School in 1836, and graduated from Harvard College in 1844. In 1845 Gould visited Europe to make himself acquainted with the instrumental equipments of the principal Observatories. With this object he spent about three months at the Royal Observatory, Greenwich; four months at the Paris Observatory; a year at the Berlin Observatory; four months at Altona; and a month at Gotha. He was thus brought into contact with most of the leading astronomers of the time, and made many lasting friendships.

Dr. Gould returned to America in 1848. In 1852 he joined the staff of the Coast Survey, and was employed as "Assistant in Charge" on many important determinations of longitude. The Reports of the Superintendent of the United States Coast Survey, 1852—66, contain many valuable papers by Dr. Gould on points connected with longitude determinations. In 1855 arrangements were made between the Trustees of the Dudley Observatory and the authorities of the U.S. Coast Survey, which it was thought would prove mutually advantageous, and Professors Bache, Henry, Pierce, and Dr. Gould were appointed members of the Scientific Council to superintend the work of the Observatory. In 1855, Dr. Gould visited Europe and obtained a new Transit Circle from Messrs. Pistor and Martins, of Berlin, and some other instruments for the Observatory. A new Heliometer was also ordered from Mr. Spencer, of Canastota, New York, but this instrument was never completed. On December 19, 1857, Dr. Gould was appointed director of the Dudley Observatory, but his relations with the trustees were not satisfactory, and in June, 1859, his connexion with the Observatory ceased. In 1866, Gould took charge of the Valencia end of the telegraphic determination of the difference in longitude between that station and Newfoundland; and the Astronomer Royal, Airy, afforded facilities for the connexion of Gould's station with Greenwich. About the same time, Gould reduced the observations made by D'Agelet, at Paris, 1783—85, with a Bird quadrant. The reductions were necessarily differential, but they afforded positions of about 2907 stars, some of which had not been observed by Bradley, and which were valuable

for the determinations of proper motions. In 1866 Dr. Gould also became deeply interested in the applications of photography to astronomy. He measured and deduced the right ascensions and declinations of about 50 stars from some photographs of the Pleiades taken by Rutherfurd, and showed that the results thus obtained agreed closely with Bessel's Heliometer measures. In 1870, on the eve of his departure for Cordoba, he again took up this question and obtained the relative positions of the stars on Rutherfurd's plates of the cluster Præsepe. Dr. Gould was so encouraged by his success that he made arrangements for the regular application of photography at the Cordoba Observatory. This work was, however, seriously interfered with by the breaking of his photographic object-glass in transit, and secondly, when a new object-glass had been secured, by personal difficulties and the pressure of other work ; but some valuable results were obtained.

The establishment of the Observatory at Cordoba, and the work executed there by Dr. Gould and his assistants, must be regarded as the most important astronomical work of his life. These works include the 'Uranometria Argentina,' the 'Zone Catalogue,' and the 'Argentine General Catalogue,' for the epoch 1875. The 'Uranometria" gives "the brightness and position of every fixed star, down to the seventh magnitude within 100° of the South Pole," with an atlas consisting of fourteen maps exhibiting on a stereographic projection the position of the stars to the seventh magnitude. The magnitudes are based fundamentally on Argelander's scale. The 'Uranometria' was published in 1879. The positions of the stars in the General Catalogue are generally fixed by several observations and are accurate results; this Catalogue was published in 1886. The volumes of the Zone observations were passed through the press, after Dr. Gould had left Cordoba, by his successor, and the last volume appeared only a short time before Dr. Gould's death.

The value of the work which Dr. Gould was enabled, by his own energy and the devotion of his assistants, to carry out whilst resident in Cordoba has received the fullest recognition of his contemporaries and has placed him in the first rank of practical astronomers. But besides work of this class, Dr. Gould established, in 1849, the 'Astronomical Journal,' of which he continued the editor till 1861, when its publication was suspended by the war; but after his return from Cordoba this Journal was re-established, and is at present continued by his friends Dr. Chandler and Professors Asaph Hall and Lewis Boss.

Dr. Gould married in 1861, Mary Apthorp Quincy, daughter of the Hon. Josiah Quincy; Mrs. Gould died in 1883, and to her memory the 'Zone Catalogue of Stars' is dedicated. Dr. Gould's

death, which occurred November 26, 1896, followed an accidental
fall down some steps.

There is a list of 82 papers by Dr. Gould, in the ' Catalogue of
Scientific Papers,' published by the Royal Society.

Dr. Gould was a Foreign Member of the Royal Society, a
Foreign Member of the Royal Astronomical Society, a Correspond-
ing Member of the French Academy of Sciences, and a member of
very many other learned societies. In 1883 he received the Gold
Medal of the Royal Astronomical Society.

<div align="right">E. J. S.</div>

EDWARD BALLARD, who died on the 19th January, 1897, will be
identified always with the Central Public Health Authority of this
country, the scientific repute of which his labours have done so
much to enhance. As a sanitarian, however, Ballard's own reputa-
tion was assured ere he joined, in 1871, the Medical Staff of the
Privy Council Office. At that date he had for sixteen years held
office as Medical Officer of Health for Islington, exhibiting through-
out abilities, investigatory and administrative, which had served to
render him conspicuous among the representatives of English sani-
tary knowledge and practice. In Ballard, Simon, then Medical
Adviser to the Government, was not slow to recognise a man of the
type essential for his purpose in building up the future Medical
Department of the State. It was Simon's business, no doubt, at
that juncture, to discover, as inspectors, men capable of developing
into the Ballards, Buchanans, and Netten-Radcliffes of after-days.
Nevertheless that he did discover these men, and as well secured
their services, must redound ever to his credit in the annals of
English sanitary progress.

Ballard was born in 1820 at Islington, a parish with which
throughout his seventy-six years of life he remained closely asso-
ciated. Here, at Islington School, he received his early education;
here he was apprenticed, his " master " being parochial surgeon and
workhouse medical officer; here, for many years, he practised as a
physician, both before and after appointment as District Medical
Officer of Health; and here also he resided during the whole of his
twenty years of Government service.

At the early age of seventeen, Ballard commenced, as apprentice,
his medical career; at nineteen he entered the Medical School of
University College. Here he seems to have at once exhibited an
industry in the accumulation of facts and an aptitude for exact obser-
vation of altogether exceptional order, qualities of mind which then,
as in after life, rendered his work always so entirely trustworthy.
His career at University College and at the University of London
was a distinguished one. He graduated M.B. in 1843, and M.D. in

1844, winning at each of his several examinations honours in the way of scholarships, exhibitions, and gold medals. He became, as a consequence, Medical Tutor to, and later on was elected Fellow of, University College.

For ten or a dozen years subsequent to graduation, Ballard practised as a physician, though at the same time exercising tutorial functions and writing on professional subjects. At this stage of his career he joined the Medico-Chirurgical Society, serving the Society as Referee of Papers, as Councillor, and finally as Vice-President. But it was not until 1856, when he accepted the post of Medical Officer of Health for Islington, that he entered on what proved to be his life-work.

In those days the sanitary functions alike of health officer and of local authority were ill-defined or not defined at all. Ballard had to educate himself, and to educate also his masters, in matters pertaining to public health; and this, on his appointment, he at once proceeded to do, with that singleness of heart and ignoring of self-advantage which throughout his public life distinguished the man. Thus, while inculcating on his authority the broad and general lines on which they should proceed in remedying the sanitary shortcomings of their district, Ballard was hour by hour studying in infinite detail series on series of facts, local and other, the right apprehension of which was, as he was very sure, necessary to fit him as a trustworthy and far-sighted adviser in the interests of the health of his district. It was in this way that Ballard made commencement at Islington of those studies of his respecting the influence of various trades on health, and respecting the relation of " Sickness " to Mortality, which subsequently, when in Government service, he extended to the whole country, with such credit to himself and to the Department of State which he served.

In further illustration of his foresight as to questions of public health likely to arise in the future, may be cited his essay ' On Vaccination : its Value and alleged Dangers.' Therein he elaborately discussed, with prescience almost, the very considerations which have only lately been occupying, for a series of years, the attention of a Royal Commission; and he delivered, in 1868, practically the same judgment on the subject as that announced in 1896 by the Royal Commission in question.

Ballard's concern for problems of the future did not, however, render him in any degree insensible to the day-by-day demands of the present, so far as his district was concerned. Current events, in their etiological and in their administrative aspects, obtained from him always their full share of attention; as, for instance, the now historic Islington outbreak of " milk-enteric fever," in 1870. At that date, notwithstanding previous observa-

tion by Dr. Michael Taylor, of Penrith, respecting milk-conveyed infection, little was with certainty known on the subject, and Ballard himself was (as he has told the writer) altogether sceptical as to any real relation between the fever and the suspected milk-service. Nevertheless, he set to work in his customary fashion to collect all the facts for and against causation of the fever outbreak by means of milk ; and, as a result, not only did he convince himself that in the particular instance the suspected milk had actually disseminated the poison of enteric fever, but also he formulated in this connexion such an array of circumstantial evidence as to afford to other minds presumption short only of absolute proof that the particular milk had had chief concern in the outbreak. His report on the subject has necessarily served as a model to later investigators.

The limits of space proper for a " notice " such as this forbid anything beyond mere attempt at indicating what sort of a man Ballard was. The work that he did in afterdays under the Local Government Board needs not to be set out. He will be known hereafter, as has been well said of him by Simon, as one of the chief confirmers and extenders of the sanitary science of his age; his researches on effluvium nuisances, on food-poisoning, on infantile diarrhœa, on epidemic pneumonia, and on a variety of matters etiological and administrative, are duly recorded in the chronicles of the Medical Officers of the Privy Council and Local Government Board. As regards his work, official and post-official, in later years, it may be remembered that much of it was done in defiance of increasing bodily infirmity. Intellectually, Ballard remained ever young, and bodily infirmity could not discourage or curb his efforts to master the unknown. After retiring from office he continued, as a labour of love, his researches into the etiology of diarrhœal disease, and he left, under his will, the data thus accumulated by him to the Medical Officer of the Local Government Board. On this subject he was still working day by day when attacked by capillary bronchitis, which proved fatal in the short space of two days.

Ballard was destitute of personal ambition. He was content with his work, and with appreciation of that work by those competent to judge of it. He sought neither honours nor emolument. From Government he obtained no honours at all; and such recognition as his work brought him from Medicine and from Science was but tardily bestowed.

W. H. P.

ALEXANDER HENRY GREEN, M.A., F.R.S., F.G.S., Professor of Geology in the University of Oxford, was born at Maidstone, October 10, 1832. He was the son of the Rev. Thomas Sheldon Green, formerly Fellow of Christ College, Cambridge, who at the

time of the birth of his son Henry was head master of the Grammar School, Ashby-de-la-Zouch.*

Green received his early education in his father's grammar school at Ashby; and the man who originally inspired him with his life-long regard for geology was the Rev. W. H. Coleman, one of the masters in the Ashby School. Coleman and his pupil were fortunate not only in living in a district of exceptional geological interest, but also in the circumstance that a few years previously (1834) Mr. Mammat, a local geologist, had made the structure of the neighbourhood classic in his well-known work 'Geological Facts.' Coleman, who seems to have been not only a scientific enthusiast but a man of singularly loveable character, died young. His memory was fondly cherished by his pupil, who always spoke of him in after years with great personal affection, and dedicated to his memory his own work on 'Physical Geology.'

In the course of a few years Green made himself so familiar with the structure of the Ashby country that when Professor (afterwards Sir Andrew) Ramsay paid a visit for the purpose of examining the district on behalf of the National Geological Survey, he was at once referred to Green as the acknowledged local authority on the subject. Ramsay soon convinced himself of the young man's remarkable scientific abilities; and, always on the look out for exceptional geological talent, suggested that if he desired to devote his life to geological work he should become a candidate for a post upon the National Survey.

From Ashby School, Green proceeded to Caius College, Cambridge. In 1855 he was placed sixth among the Wranglers in the Mathematical Tripos, and was elected a Fellow of his College in the same year. During his tenure of his Fellowship he took pupils in mathematics at his own College, and afterwards held masterships in mathematics at two private schools.

But although mathematics had gained for him his high position in the University, and the teaching of mathematics had a special charm for him, yet the science of geology had obtained too strong a hold upon his mind and his sympathies to be relinquished. The love of the science which had been awakened in his early life by Coleman, and his own keen interest and pleasure in the pursuit of personal geological investigation had both become intensified and fixed at Cambridge by the eloquent teachings of Sedgwick; and in 1861 Green applied for, and was appointed to, a post on the Geological Survey of England and Wales. His connexion with the

* I have to thank Professor T. G. Bonney, F.R.S., Mr. H. B. Woodward, F.R.S., Mr. W. W. Watts, M.A., and others, for information and kindly assistance in the preparation of this memoir.

Survey lasted from 1861 to 1874. He ranked as an assistant geologist from 1861 to 1867, and as a geologist to the close of his Survey career.

During the time of his connexion with the Survey, he was engaged first in mapping the Jurassic rocks in the Midlands, and afterwards in surveying the Carboniferous rocks in Derbyshire, Yorkshire, and the bordering counties. The broader results of his field work became embodied in a large number of published maps of the Geological Survey, some upon the 1-inch and some upon the 6-inch scale. But several detailed survey memoirs, descriptive of the country surveyed by himself and his colleagues, were written wholly or in part by him. Among these may be mentioned "The

and his colleagues on the Survey, and it contains one of the best descriptions of the British coal measures in the language.

From this time forward Mr. Green became naturally regarded as one of the leading British authorities upon all geological matters connected with coal and coal mining, and the relationship of geological structure to economics in general.

In 1870 he resigned his post on the Survey, having been appointed Professor in Geology in the newly founded Yorkshire College at Leeds. Students of geology, however, were few in number, and funds were not superabundant; but Green's energy and abilities found ample scope, for, in addition to holding the geological chair, he was appointed to the Professorship of Mathematics in the same College, and he acted also as lecturer upon the subject of Physical Geography.

Professor Green's connexion with the Yorkshire College ceased in 1888, when, upon the resignation of Professor Prestwich, he was appointed to the Chair of Geology in the University of Oxford. He threw himself with his characteristic energy heartily into his new work, giving not only the ordinary courses of lectures and laboratory

instruction, but holding special excursion classes and delivering occasional courses of lectures to extra-University students.

Early in 1895 he suffered from a severe attack of influenza. From this he never seems to have fully recovered, and in his enfeebled state the ceaseless strain of work and responsibility soon began to tell severely upon his general health. Early in August, 1896, he had a paralytic stroke. This affected his right side and confined him to bed; but he wrote to his friends cheerfully, through an amanuensis, that he was making good progress, and hoped before many weeks were over to be at work again. But these hopes were, alas, illusive, and a second attack on August 19 proved fatal.

Professor Green was M.A. of both Cambridge and Oxford, and a Fellow and Vice-President of the Geological Society of London. In 1886 he was elected a Fellow of the Royal Society, on the Council of which he served in 1894 and 1895. In 1890 he filled the office of President of Section C (Geology) at the meeting of the British Association at Leeds. He was Examiner in Geology to the Universities of Durham and Cambridge, Examiner for the Home and Indian Civil Services, Assistant Examiner in Physiography to the Science and Art Department, and, at the time of his death, one of the Examiners in Geology for the University of London.

Professor Green contributed occasional original papers to the Geological Society and to various scientific magazines. Among these may be mentioned his papers on the "Carboniferous Rocks of the North of England" and on "Sub-aërial Denudation," "The Geology of Donegal" and "The Geology of the Malvern Hills." But his publications in this department of geology, when compared with those of many of his geological contemporaries, are relatively few and insignificant. On the practical side of the science, however, there were not many who accomplished so much or whose reputation was so widespread or so well deserved. His life-long acquaintance with the details of geological structure, his clear head and his sound judgment, rendered his advice on matters of engineering geology of great value; and, as a consequence, his services were being continually called into requisition in economic undertakings, especially those in reference to coal mining and to water supply. He was engaged in many of the most important operations of this nature carried out in Britain during the last thirty years. He also visited parts of South Africa in the pursuit of his practical work, and in this way he was able to obtain a considerable insight into its little-known geology. The paper in which he brought his African results before the Geological Society is one of the most important contributions yet made to the subject.

Professor Green wrote papers upon the Yorkshire coal measures in a volume of 'Essays upon Scientific Subjects,' issued by the

Professors of the Yorkshire College, and he published a popular
little work upon the 'Birth and Growth of Worlds.' But the book
by which Green was most widely known among geologists and lovers
of geology was his 'Manual of Physical Geology for Students and
General Readers,' the first edition of which was published in 1876,
and which attained to a third edition in 1883. This manual is
admittedly the best English work in this branch of the science. It
is remarkably typical of its author, thoroughly practical and almost
painfully conscientious; it is lucid and modest, but at the same time
original and bold; and it has done more, perhaps, than any other
work to foster a wide appreciation of physical geology. In this
work we see Green at his best. His deep love for his science and its
students is evident in every chapter. His delight in the beauties of
literature and in good literary style gives a peculiar charm to the
book as a whole; and the mathematical bent of his mind is evident
in the crystallographic part of the work and in his original methods
of representing outcrops of strata and the like, methods which have
subsequently proved of especial utility in the advance of geological
science.

Professor Green's geological teaching in his lectures, though
not perhaps calculated to move to enthusiasm, was exact and
thorough. His lectures were usually illustrated by careful experi-
ments, and were directed less to the presentation of geological facts
and accepted theories than to the inculcation of the true scientific
method of research and habit of reasoning. But as an original
worker in and as a teacher of practical field geology Green had few
equals. His own life training, his special artistic abilities, and his
love of detail all conspired to this end; and there can be no question
that it is to his labours and teaching that British stratigraphical
geology owes much of its present influence.

In the death of Professor Green not only has Oxford University
lost a most distinguished member of its professorate, but geology
has lost a steady and fruitful worker, one who loved the science
entirely for its own sake, and whose life was spent in quietly labour-
ing for its advancement. His wide knowledge of literature and of
general science, and his deep and unbiassed appreciation of all modes
of their progress, made him a most pleasant companion; while his
calm judgment and his cautious habit of mind rendered him an
invaluable colleague. Always frank and fearless in the expression
of an opinion at which he himself had arrived, he was, nevertheless,
so hearty, so genial, and so unselfish that his loss will be most
keenly felt, not only by all his fellow workers in the science, but by
a still wider circle of loving friends.

<div align="right">C. L.</div>

EDWARD JAMES STONE was born in London on the 28th February, 1831. As a child his constitution was delicate, so that soon after entering the City of London School his health broke down, and he was sent to the country for several years, where he was educated at a private school until ready to enter King's College, London.

Although his higher education only began at the age of 20, he took a scholarship at Queens' College, Cambridge, in 1856, whence he graduated as Fifth Wrangler in 1859, and was immediately elected to a Fellowship. The following year he was selected for the important position of Chief Assistant at the Royal Observatory, Greenwich, his predecessor in that post, the Rev. R. Main, having been appointed Radcliffe Observer at Oxford.

With what diligence he applied himself to the duties of his office and how wide a view he took of his responsibilities, is made evident by the series of important papers which he soon afterwards began to communicate to the Royal Astronomical Society.

His work was obviously congenial, he had a marked inborn capacity for dealing with large masses of figures, a high estimate of the practical importance of the work on which he was engaged, sound mathematical training, and an almost impatient desire to derive from the long series of Greenwich observations results which would be of immediate value to science.

It is not difficult to trace the origin of the line of research which Stone subsequently followed with such zeal and pertinacity. Fresh from study of the lunar and planetary theories, so far as these subjects were treated in his Cambridge curriculum, the attention of the young astronomer was early arrested by the interesting problems which were then opening up in consequence of the researches of Hansen and Le Verrier.

We have a statement of the *motif* of Stone's first work in the introductory paragraphs of his first astronomical paper,* "Determination of the Solar Parallax from N.P.D. Observations of Mars at Greenwich and Williamstown."

" In his tables of Mars, published in the 'Annals of the Imperial Observatory,' Paris, 1861, M. Le Verrier remarks that it is impossible to reconcile the observations of Mars with theory without attributing to the perihelion a motion greater than any which can be obtained except by a sensible increase of the received planetary masses ; that the necessary agreement between theory and observation could be obtained by increasing the received value of the mass of the earth in proportion to the sun's mass by not less than a tenth part, but that such an increase in the received value of the earth's mass would necessitate a corresponding increase in the received value of the sun's mean equatorial horizontal parallax of a thirtieth part.

" M. Le Verrier deduced the same result from a discussion of the latitudes

* 'Monthly Notices R.A.S.,' vol. 23, 1863, p. 183.

and motion of the node of Venus. The difficulties raised in the theory of Mercury, although not removed, were slightly diminished by the same increase of the earth's mass.

"In his solar tables, M. Le Verrier has adopted the value 8·95" for the mean equatorial horizontal solar parallax, this value was obtained by determining from observation the coefficient of the lunar equation, and assuming the mean lunar parallax and data furnished by the theories of precession and nutation.

"The way in which M. Le Verrier has thus evolved from the theories of Venus, the Earth, and Mars, the necessity of the value of the mean solar parallax much greater than the usually received value 8·57", and not differing greatly from 8·95", must render it extremely probable that the true value of the sun's mean parallax does not differ greatly from that quantity."

Stone then proceeds to discuss the Greenwich and Williamstown observations, and derives a value of 8·932" for the solar parallax.

In the 'Monthly Notices' for May, 1863, Stone remarks that in the lunar theories of Plana, Pontécoulant, and Lubbock the coefficient of the parallactic inequality deduced with the usually received values of the involved constants amounts to 122·1", which, if we increase the value of the mean solar parallax by a thirtieth part, becomes 126·2", coinciding closely with the observed values as derived by Airy, viz.:—124·7" from meridian and 125·5" from altazimuth observations. He suggests that it would be a point of interest to determine whether Hansen's lunar theory would bear any considerable increase in the mean solar parallax.

Stone had apparently overlooked a letter of Hansen's,[*] in which the writer says :—

"The coefficient of the parallactic equation I found to be

$$125·705",$$

"an amount exceeding any which has hitherto been assigned, and which indicates a greater value of the sun's parallax than has been deduced from the observations of the transit of Venus. The Greenwich observations, exclusive of any others, assign the foregoing value of the parallactic inequality, and the Dorpat observations nearly the same value. I cannot, therefore, alter it."

In the following number of the 'Monthly Notices,'[†] Hansen, in reply to Stone, refers to the above quoted paragraph, and in the same periodical for November, 1863, gives with more detail, as the result of his researches, the value 8·9159" for the mean solar parallax.

Till the note of alarm was thus sounded by Hansen in 1854, and echoed by Le Verrier in 1861, astronomers had almost universally

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[*] 'Monthly Notices R.A.S.,' May, 1854.
[†] June, 1863.

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accepted as definitive the value of the solar parallax found by Encke from his discussion of the Transits of Venus of 1761 and 1769, viz. :—

$$8·57''.$$

Winnecke, who proposed the practical programme for the observations of Mars in 1862, discussed the thirteen corresponding observations made at Pulkowa and the Cape,* and found for the solar parallax

$$8·964''.$$

This, together with Stone's result already quoted, seemed to prove, from the practical as well as from the theoretical side, that the then accepted value of this fundamental constant of astronomy was probably at least one thirtieth part of its amount in error.

The question of the exact redetermination of the solar parallax became at once one of supreme interest, and, as Airy put it, " the noblest problem in astronomy."

Stone threw himself into its discussion with his characteristic vigour. In the 'Memoirs of the R.A.S.'† he discusses afresh the meridian observations of Mars made at Greenwich in 1862, combining them with those made at Williamstown and the Cape of Good Hope, and derives for the solar parallax

$$8·945''.$$

In May, 1865, he communicated to the R.A.S. an important memoir on the " Constant of Lunar Parallax," in which he derived the value of that constant, as defined by Adams, from a series of observations of the moon made at the Cape Transit Circle in the years 1856–61, combined with corresponding observations made at Greenwich. Up to the present time Stone's result is accepted by astronomers as the most reliable direct determination of this important constant.

In the ' Monthly Notices ' for April, 1867, Stone calls attention to a slight numerical error in the computation of the value of the mass of the moon which was employed by Le Verrier in his derivation of the solar parallax from the known relations between the parallaxes of the sun and moon, the mass of the moon, and the lunar equation.‡ With this revised value of the moon's mass (computed from Le Verrier's adopted values of the constants of precession and nutation) and with the value of the lunar equation (6·50'') derived by Le Verrier from his discussion of the existing meridian observations of the sun, Stone derives for the solar parallax the value

$$8·91''.$$

* ' Ast. Nach.,' 1409, April, 1863.
† Vol. 33, May, 1864.
‡ ' Annales de l'Observatoire de Paris,' vol. 4, p. 101.

In two notes* Stone reverts to the determination of the moon's mass, and, adopting 50·377″ for the luni-solar precession, and 9·223″ for the constant of nutation, he derives for the moon's mass the value

$$\frac{1}{81·36}.$$

Following the same general line of research, Stone then under-takes a direct determination of the constant of nutation based on Greenwich N.P.D. observations of Polaris, 51 H Cephei, and δ Ursæ Minoris, and the R.A. observations of Polaris made with the transit circle 1851–65. The preliminary results of this work are published in the 'Monthly Notices,' vol. 28, 1868, p. 229, and vol. 29, 1869, p. 28. The complete discussion appears in the 'Memoirs of the R.A.S.,' vol. 37; the resulting value of the constant of nutation is

$$9·134″.$$

In the concluding portion of this memoir, Stone draws attention to the strong evidence which his discussion gives of a periodic change of latitude, a subject which, had he followed it up, might have led to an earlier discovery of Chandler's now well-known law of variation of latitude, but his thoughts were otherwise occupied.

In the supplementary number of the 'Monthly Notices' for 1868, Stone communicated to the Royal Astronomical Society his "Re-discussion of the Observations of the Transit of Venus of 1769," in which, after reverting to the above-quoted evidence in favour of an increase in the accepted value of the solar parallax, he proceeds to rediscuss the observations made at five stations where internal con-tacts of Venus with the sun's limb were observed both at ingress and egress by ten observers. An independent interpretation was put upon the language employed by each observer to describe the phenomena which he noted at different instants of time, and the assumption was made that such phenomena could be divided into two distinct classes—viz., *true* and *apparent* contacts—separated by a definite interval of time. This interval (assumed to be constant for all observers) was introduced in symbolical form into all the equations. The solution of the equations so formed led to the value

$$8·91″$$

for the solar parallax, with the estimated probable error

$$± 0·02″.$$

The observations were there represented as follows :—

* 'Monthly Notices R.A.S.,' vol. 28, pp. 21 and 42.

Observer.	Computed minus observed.	Station.	Computed minus mean.
Hell..........	−1·7ˢ ⎫	Wardhus......	+0·6ˢ
Sajnovics	+2·8 ⎭		
Wales	+0·4 ⎫	Hudson's Bay	−0·1
Dymond	−0·6 ⎭		
Chappe	+0·5 ⎫		
V. Doz	−0·4 ⎬ St. Joseph		−1·6
Medina	−5·4 ⎭		
Green ....:	+5·8 ⎫	Otaheite	+0·8
Cook	−4·2 ⎭		
	−0·9	Kola	−0·9

This is not the place in which to enter upon a detailed criticism of the above discussion; nor, with the information now at our disposal from the subsequent Transits of Venus of 1874 and 1882 and other more recent sources, would it be fair to do so. Although it is now certain that the above value of the solar parallax is about *one per cent.* too great, and that the representation of the observations conveys an impression of accuracy which is far greater than that obtainable in such observations, it is but justice to Stone's memory to quote the opinion held at the time by astronomers of his own country in regard to this work. In February, 1869, on presenting the gold medal of the Royal Astronomical Society to Mr. Stone for his "Rediscussion of the Observations of the Transit of Venus in 1769 and his other contributions to Astronomy," the President, Admiral Manners, said :—

"By this important investigation, Mr. Stone has earned for himself the gratitude of astronomers of all countries. He has shown, beyond all doubt, that the method pursued by his illustrious countryman Halley, when fairly treated, is capable of furnishing a value of the solar parallax commensurate in precision with the expectations formed of it.

"But this is not all. Mr. Stone, by his researches in this instance, has wiped from astronomy a reproach, which did not indeed legitimately attach to it, but which only one of those intellectual triumphs which from time to time have adorned the annals of our science was capable of extirpating."

In order to illustrate the sequence of Stone's researches, we have thus far recorded only such of them as bear upon the determination of the solar parallax and its allied constants. We must now return to other works which he carried out during the period that he was chief assistant at Greenwich. Amongst the most important of these was the large share which he took, under Airy's direction, in *the preparation* of the Greenwich seven-year catalogue of 2022 stars *for the equinox* 1860.

Throughout his whole life, one of Stone's most characteristic qualities was his high sense of responsibility and strict regard to official duty. However absorbing may have been the independent researches in which he was engaged, his official duties were at all times his first consideration. These occupied not only his official hours at the Observatory, but he gave to them and to strictly allied work much of the labour of his private time.

In the Greenwich catalogue for 1850, Airy employed the very unsatisfactory proper motions of the British Association Catalogue of Stars. For the formation of the 1860 catalogue the proper motions determined by Main* were available. Recognising the importance of Main's work, Stone continued it, and, in the 'Memoirs of the R.A.S.,' 1864, vol. 33, gave, for 460 stars of the seven-year catalogue, proper motions computed by reducing Bradley's observations (as derived in Bessel's *Fundamenta*) to the equinox of 1860, and comparing the results with the corresponding places of the 1860 catalogue.

A further proof of his deep interest in his official duties is given in his paper "On the Accuracy of the Fundamental Right Ascensions of the Greenwich Seven-year Catalogue for 1860," published in vol. 34 of the 'Memoirs of the R.A.S.,' where he specially discusses the accuracy of the fundamental right ascensions of γ Pegasi, β Geminorum, α Virginis, and α Aquilæ.

In November, 1867, Stone communicated to the Royal Astronomical Society a paper "On Bessel's Mean Refractions," in which he showed that the tabular refractions of Bessel's *Fundamenta* were too great, and required to be diminished by one five-hundredth part, in order to represent the Greenwich observations of circumpolar stars made in the years 1857–65. This important conclusion has since been fully corroborated at the Washburn Observatory, in the United States, by Comstock in his determination of the constant of aberration, and also by a discussion of the Cape and Greenwich observations.† Nyrén's discussion of the Pulkowa refractions also points to the same conclusion.

Besides the already mentioned important papers connected with well-marked lines of continuous research, we find no less than twenty notes of a more miscellaneous character, communicated by Stone to the Royal Astronomical Society. These papers testify to the wide interest which he took in all contemporary astronomical research during the ten years he remained Chief Assistant at Greenwich.

In the early part of 1870 Sir Thomas Maclear resigned his post as Her Majesty's Astronomer at the Cape, and in June of the same

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* 'Memoirs R.A.S.,' vols. 19 and 28.

† Introduction to the 'Cape Catalogue' for 1885.

year Stone was appointed his successor. The thirty-six years of Maclear's directorate had been fruitful in many ways. An arc of meridian had been measured, a splendid series of extra-meridian observations secured, and the greater part of all this work had been reduced and published. Maclear's inborn tastes had drawn him from a lucrative profession and forced him to become an astronomer. He was by nature of an enthusiastic disposition and an ardent observer. The clear skies at the Cape offered him ample opportunity to accumulate observations, but he was never provided with a staff sufficient to overtake arrears of reduction. He had therefore to choose between limiting the number of observations or leaving many of them unreduced and unpublished whilst he secured as many more as he could. He chose the latter alternative, and thus of the long series of meridian observations made during the thirty-six years of his directorate nothing had been published except the meridian observations of 1834 and such star places as were required for comet comparison stars, moon culminators, &c.

Stone had for a long time recognised the special importance of forming an accurate and extensive catalogue of southern stars, and had even endeavoured to persuade Airy to extend the range of magnitude of stars observed at Greenwich, and to construct a catalogue of northern stars complete to some such order of magnitude as the 7th. In his introduction to the ' Cape Observations of 1871, 1872, and 1873 ' Stone states : " The chief inducement which led me to accept the appointment was the opportunity which the position afforded for the formation of a general catalogue of southern stars to about the 7th magnitude." It was therefore with enthusiasm and high resolve to construct such a catalogue that Stone betook himself to the Cape. But his official instructions were imperative on one point, viz., that he was to render the large number of meridian observations accumulated by Maclear available for the use of astronomers with as little delay as possible.

Such an instruction was one which would have discouraged most men.

" I found myself," says Stone,* " with a very limited staff, unexpectedly confronted with the result of thirty-six years of miscellaneous observing in all states of reduction, nothing completed, and nothing which could be brought forward for publication and use without a very considerable expenditure of time and skilled labour. I fear the course pursued of continuous miscellaneous observing without reduction has not tended to the advancement of accurate astronomy to any extent proportional to the labour expended upon the work, and still required to be expended upon it, before the results can be rendered useful. Such observing is rarely conducted in a way to facilitate the subsequent reductions or to economise

* Introduction to the ' Cape Catalogue ' for 1860.

labour in observing. This will be apparent to anyone who will count the number of observations of stars between 67° and 117° north polar distance, and consider that a catalogue formed from the results of other years would contain observations of these stars to very nearly the same relative extent. Of the large number of observations accumulated here from 1832 to 1855 with the transit instrument and mural circles, the places of the southern stars, out of reach of northern observatories, will, when reduced, still be of value for proper motions, but the immense number of observations of well-known stars made here with the old instruments can now, I fear, never repay the labour required for their reduction. . . . . I have made these remarks, not only in justice to the present staff, and to explain the work upon which they have been employed, but because it was these considera-tions which led me to pass over the earlier observations, and to commence the systematic reductions with the year 1856, when the transit circle was first brought into regular use. I felt that these reductions could not be any longer delayed without the value of the results being greatly diminished. I had, and still have, hopes that the data collected in the present cata-logue for corresponding observations at the northern observatories would be found sufficient to meet the actual requirements of astronomers so far as these requirements can be met by the material collected here, and that I might be relieved from the laborious and somewhat useless task of com-pleting the reductions of the earlier observations of stars whose positions have been fixed already with an accuracy greater than could be expected to be attained in the observations made with the, comparatively speaking, inferior instruments in use at this observatory before the introduction of the transit circle."

We dwell somewhat on this point because inquiry has shown that these words convey precisely Stone's frequently expressed views, and we can thus more fully admire the high sense of duty which prompted the self-sacrifice and devotion with which he applied himself to the subsequent execution of an uncongenial but honour-able task.

The meridian results for the year 1856 were published by Stone in 1871, for the years 1857 and 1858 in 1872, and those for 1859 and 1860 in 1874. The general catalogue of 1159 stars, derived from all these observations and reduced to the equinox of 1860, was published in the year 1873.

In the interval of his regular labours Stone next devoted his attention to the examination and publication of the results of obser-vations made with the transit instrument and mural circle in the years 1834 to 1840. Maclear had already reduced the whole of these observations, and Stone accepted these results generally as satisfactory, but he redetermined the azimuthal errors and re-reduced the observations of the close circumpolar stars. The work of exami-nation occupied Stone from time to time as opportunity offered, and " much progress was made in it during the comparative leisure enjoyed during my visit to England in 1875." The results were

finally published by Stone in 1878, in the form of the 'Cape Catalogue of 2892 stars reduced to the equinox 1840.'

But Stone did not allow these labours to interfere with his main object. Within a month of his arrival at the Cape a working list of stars within 5° of the South Pole had been prepared, and observations with the transit circle were commenced. The work was prosecuted with systematic vigour notwithstanding the loss of two assistants—Mr. Sinfield, who died in September, 1871, and Mr. Mann, his chief assistant, who, after a year's illness, died in April, 1873. Although there was considerable delay in replacing these valued assistants, Stone was able to publish in 1876 the annual results of the Cape observations for 1871, 1872, and 1873, containing accurate places of all Lacaille's stars within 15° of the South Pole, and of nearly all the stars to the 7th magnitude within the same zone. At the same time he was able to announce that the similar stars within 35° of the South Pole had already, in December, 1875, been observed, and arrangements made for the observation of the next zone, 135° to 145° N.P.D., in the year 1876; that in the year 1877 the work, if persevered with, should overlap that of some of the northern observatories, and with the zone 115° to 125° N.P.D., it might perhaps be brought to a close in 1878. This programme was fulfilled to the letter, and the observations of the zones were completed in the end of 1878.

Meanwhile a large stereographic projection of the southern hemisphere had been prepared, and upon it were projected the places of all the stars which had already been observed, and wherever *lacunæ* appeared within the limits of N.P.D. 115° to 180°, efforts were made during the first four months of 1879 to fill them up by observing stars of rather a lower magnitude than the 7th of Lacaille's scale. During the same period also such control observations as seemed necessary were taken, and the whole work of observation was completed.

The reductions had been rigorously kept up to date, and before the end of May, 1879, the whole of the star places had been reduced to the equinox of 1880, the means taken and the precessions and secular variations computed.

The Rev. Robert Main, Radcliffe Observer at Oxford, died on the 9th of May, 1878. Stone, having nearly fulfilled the object for which he came to the Cape, became a candidate for the post, and was soon afterwards elected to it. The Radcliffe Trustees yielded to the request that Stone might be allowed a year to complete his work at the Cape, and the Rev. Charles Pritchard, then Savilian Professor of Astronomy at Oxford, was appointed *interim* director. On the 27th May, 1879, Stone sailed from the Cape, taking with him all the documents necessary for the preparation of his great southern catalogue for press.

At Oxford, Stone applied himself to this work with such energy, that in 1881 the Cape Catalogue of 12,441 stars for the equinox 1880 was passed through the press, and its publication was welcomed by astronomers as one of the most important contributions ever made to sidereal astronomy.

His personal welcome amongst his colleagues was no less cordial. He was at once elected a Vice-President of the Royal Astronomical Society, and, on the vacation of the presidency by Mr. Hind, Stone was elected to the chair.

We must now briefly review Stone's other labours during his stay at the Cape. In 1871 he published an experimental determination of the velocity of sound, based on chronographic determinations of the interval which elapsed between the flash of the Cape Town time gun and the instant when the noise of the report reached the Cape Observatory. Various papers on the theory of probabilities, including a criterion for the rejection of discordant observations, also appear from Stone's hand during this period, and there are sundry papers in the 'Monthly Notices' on proper motions of stars, observations of comets and variable stars, &c., which testify to his continued interest in 'general astronomy, notwithstanding his preoccupation in the great work of his catalogue.

In 1874 he undertook an expedition to Klipfontein, in Namaqualand, and successfully observed the solar eclipse there on the 16th April of that year. He employed a slit spectroscope, with two dense flint prisms of 60°, and was successful in observing the reversal of the Fraunhofer lines at the instant of disappearance of the sun's limb. On the same expedition he made a valuable series of magnetic observations in Namaqualand, the first series of its kind secured in that region.

In 1877 (Appendix to the Cape Observations for 1874) he published a set of star constant tables for computing the apparent places of stars from their mean places, or *vice versâ*. These tables have been largely used, first at the Cape, and subsequently at Greenwich and other observatories.

When the 'Parliamentary Report on the Telescopic Observations of the Transit of Venus made in the expeditions of the British Government' reached the Cape, Stone immediately recast the phases, recomputed the results, and by return mail communicated to the Royal Astronomical Society* the results of his rediscussion, which gave, instead of the official result (8·76″), the value

$$8·89''$$

for the solar parallax. He followed up this communication by a further paper published in the next number of the 'Monthly Notices,'

* March, 1878.

in which he included a discussion of the Cape observations, and arrived at a very similar conclusion.

At Oxford, Stone had barely finished the work connected with his great catalogue when he was nominated secretary of a committee of the Royal Society, to advise the Treasury and Admiralty with respect to the steps which should be taken for observation of the Transit of Venus, in 1882. An executive committee was appointed by Government, in 1881, and, at the request of this committee, Stone undertook the duties of " Directing Astronomer in connexion with the arrangements for the observations of the Transit of Venus, and with the subsequent deduction of the results." The Radcliffe Observatory was converted into a training school for the observers, and Stone devoted himself, heart and soul, to the organization of the various expeditions. The thoroughness with which he did this work, and the great labour which he gave to it, are only fully known to those who were connected with the expeditions. Stone's report, which is attached to that of the executive committee and published in a Parliamentary Blue Book, was issued in 1887. It gives a detailed *précis* cf all the observations, and derives from the observations of various classes of contact different values of the solar parallax.

From the ' Report of the Council of the Royal Astronomical Society for February, 1888,' the adopted value of the solar parallax from Stone's discussion of the British Transit of Venus observations, appears to be

$$8\cdot832'' \pm 0\cdot024''.$$

In 1883, Stone communicated to the Royal Society a paper entitled " The principal Cause of the large Errors at present existing between the positions of the Moon, deduced from Hansen's Tables and Observation, and the Cause of an apparent Increase in the secular acceleration in the Moon's Mean Motion required by Hansen's Tables, or of an apparent Change in the time of the Earth's Rotation."

This was soon followed by a communication to the Royal Astronomical Society of a paper entitled " On the Change of the Length of the Tabular Mean Solar Day which takes place with every Change in the adopted value of the Sun's Mean Sidereal Motion."

Stone's contention was that by the adoption of Le Verrier's Tables of the Sun in the ' Nautical Almanac ' of 1864, and in subsequent editions of the same work, for the computation of the sun's longitude and the sidereal time at mean noon, instead of Carlini's tables, previously employed for this purpose, a *per saltum* change was made in the unit of time used by astronomers, the original and cumulative effect of which, from 1864, was to change by twenty-seven seconds

the *absolute* instant of any *nominal* mean time computed in the beginning of 1883, and that this error accumulates at the rate of 1·48ˢ per annum. It was in vain that Airy, Adams, and Cayley endeavoured privately to convince Stone of the error of this conclusion, and to show him that the effect of the change in question was 1/365th part of what he claimed it to be. Adams then published a paper,* which explains the whole point at issue in the clearest and simplest manner, viz., that mean solar time is measured, not by the sun's mean motion in *longitude* (as Stone's theory supposes), but by the motion of the mean sun in *hour angle*, which is about 365 times greater in amount. Cayley, Gaillot, and afterwards Newcomb, published notes and questions intended to put the matter in a way that should convince Stone of his error. But no arguments or illustrations were of avail, and, during the last ten years of his life, almost the whole of his time which was not occupied by pressing official duty was employed in devising fresh methods of presenting his theory for discussion, or in comparing the places of the moon, as observed at Oxford, with Hansen's tables both corrected and uncorrected for the term dS given by Stone's theory. By a very remarkable coincidence the present errors of Hansen's Tables of the Moon agree within very narrow limits with the corrections dS, and it must have been this circumstance which completely blinded Stone's eyes to the true state of the case. What Stone's theory does not explain is the simple fact that if no change had been made in the 'Nautical Almanac' in 1864, or, what is the same thing, if the mean solar times derived from Carlini's (or Bessel's) tables had been used to convert the sidereal times of observed transits of the moon at Oxford into Greenwich mean time, and if the mean times so computed had been used as arguments to derive the tabular place of the moon from Hansen's tables, it would be found that the errors of Hansen's tables in right ascension would have differed but a few hundredths of a second of time from what they would have been if computed from the data of the present 'Nautical Almanac.'

On taking up residence at Oxford, Stone found that the staff consisted of but one assistant, whose time was chiefly occupied in making and reducing the regular meteorological observations and observing stars for clock error. Two assistants were added to the staff, a systematic examination of the transit circle was instituted, and regular meridian observing was begun in 1880. Plans were made for the construction of a catalogue of all stars to the 7th magnitude from N.P.D. 115° (the northern limit of his Cape zones) to the equator. Observations were secured at a rate varying from about 2000 to 3500 per annum from 1881 to 1893, these were regularly reduced and published in annual volumes, and in 1893 meridian

* 'Monthly Notices R.A.S.,' vol. 44, p. 43.

observing, except of the sun and moon and occasional stars, seems
to have been suspended during the completion of the 'Radcliffe
Catalogue' for 1890. This catalogue contains places (based gene-
rally on a minimum of three observations of each star, and many more
of clock-stars and others) of all stars to the 7th magnitude between
N.P.D. 115° and the equator; and, wherever gaps occur in
the distribution of 7th magnitude stars, such gaps have been filled
up by the observations of stars of a fainter order of magnitude.
The sun and moon were also regularly observed on the meridian, and
a considerable number of occultations, comets and double stars were
observed with the extra-meridian instruments.

In 1886 an excellent equatorial by T. Cooke and Sons, with an
object glass of 10 inches aperture, was presented to the Radcliffe
Observatory by Mr. J. Gurney Barclay, and with the aid of this fine
instrument the value of the extra-meridian work was greatly
enhanced.

In 1888 Stone co-operated with Yale, Leipzig, and the Cape in the
heliometer observations of the minor planet Iris for solar parallax,
and his staff contributed meridian observations of the planet and
comparison stars. In 1889 meridian observations of Victoria and
Sappho and of their comparison stars were also made under Stone's
direction in connexion with the solar parallax campaign of that
year.

In addition to all this astronomical work meteorological observa-
tions were systematically recorded, reduced and published. Stone
then planned the observation of similar meridian observations of all
stars to the 7th magnitude in successive zones northwards from the
equator, and the work was actually begun before his death.

Stone's reductions of Hornsby's observations of γ Draconis made
at the Radcliffe Observatory 1788-91 have been published in the
'Monthly Notices' for June, 1895, and show of what admirable
quality these zenith sector observations are. Hornsby's observations,
made with two brass quadrants of 8 feet radius and a transit instru-
ment of 4 inches aperture, next attracted Stone's attention, and
before his death he had made considerable progress in reduction of
the observations of 1774. Stone considers that these observations,
extending as they do for a number of years subsequent to 1774,
promise results of very considerable value. By thus bringing them
to light and proving their excellence, Stone has opened for his
successor a mine of scientific wealth which will certainly not be
neglected.

In 1896, Stone was a member of the party which accompanied
Sir George Baden-Powell in his yacht to Nova Zembla, and was
thus one of the few astronomers who were favoured with fine
weather for observing the total eclipse of the sun of that year.

Stone secured successful observations; the paper he wrote on the results of them was not printed before his death, but it will shortly appear in the memoirs of the Royal Astronomical Society.

Stone's scientific papers number in all about 150, and cover a very wide range; they were chiefly communicated to the Royal Astronomical Society.

Such, in brief, is the record of Stone's labours. Very very few astronomers have done more of solid and useful work for the advancement of astronomy. But for the simultaneous and almost phenomenal labours of Gould at Cordoba, it might be said of Stone that he created our knowledge of the exact sidereal astronomy of the Southern Hemisphere.

Unlike Maclear and Gould, he was not a great observer; at Greenwich he personally made about $2\frac{1}{2}$ per cent. of the observations secured there during the period 1860 to 1869; at the Cape and Oxford he made very few of the observations, but he closely super-vised his assistants, and laboured early and late at every detail of reduction and examination.

Trained in the rigid and systematic school of Airy, and gifted with remarkable powers of speed, accuracy, and endurance in com-putation, he was enabled to carry out, with a very small staff, the great record of work which he produced. He made the chief part of Maclear's meridian observations available for science, and created his two great catalogues—the Cape Catalogue of 12,441 stars for 1880, and the Radcliffe Catalogue of 6424 stars for 1890. By these works his name will be chiefly remembered. But the influence of a man so earnest and diligent as Stone was in most departments of astronomy, can never die; it will live to inspire like zeal and devo-tion in others, who are seeking truth for truth's sake, as Stone did.

After a short and painless illness, Stone died suddenly at the Radcliffe Observatory on the 9th of May, 1897, the nineteenth anni-versary of the death of Main, his predecessor.

Stone was a Fellow of Queens' College, Cambridge. In 1868 he was elected a Fellow of the Royal Society, and, as already stated, was in 1869 awarded the gold medal of the Royal Astronomical Society. He was a Corresponding Member of the Société Nationale des Sciences Naturelles, Cherbourg, and Honorary Member of the Literary and Philosophical Society of Manchester. In 1881 he received the Lalande Medal of the Paris Academy of Sciences for his Cape catalogue; in 1892 the University of Padua conferred upon him the honorary degree of Doctor of Natural Philosophy, and from March, 1883, he was a Member of the Meteorological Council, London.

D. G.

On the 29th May, 1897, died JULIUS VON SACHS, Professor of Botany in the University of Würzburg, after six weeks' illness of acute phthisis, the result of influenza. The following account of his life is based upon information, kindly supplied by members of his family, derived from a manuscript autobiography, which does not, however, extend beyond the year 1882.

Julius Sachs was born at Breslau on October 2, 1832. After quitting the gymnasium in 1845, he appears to have turned his attention to biological studies, for in 1851 he became private assistant to the physiologist Purkyně at Prague. Whilst there he entered upon a regular university course, devoting his attention to animal anatomy and physiology, and subsequently, for two whole years, to physics and mathematics. He must, however, have also been studying botany at this time, for he soon began to publish botanical papers; but it is not clear under what circumstances he pursued this study, for, though he inscribed himself for Kosteletzky's botanical lectures, it appears that he did not attend them. In 1856 he took his doctor's degree, the subject of his dissertation, which was not published, having been Diffusion. Shortly after this he finally adopted a botanical career, establishing himself in 1857 as Privatdocent for plant-physiology in the University of Prague. Whilst still at Prague, Sachs was invited by the Agricultural Academy of Tharandt, in Saxony, to write a memorandum on the importance of plant-physiology to agriculture, with the result that, in April, 1859, he was appointed physiological assistant to the Agricultural Academy there. In 1861 he was called to be the Director of the Polytechnic at Chemnitz, a post which he held for only four months, when he was transferred to Poppelsdorf, near Bonn, as the Director of the Agricultural Academy at that place; here he remained till 1867, when he was nominated Professor of Botany in the University of Freiburg-in-Breisgau. In 1868 he accepted the Chair of Botany in the Bavarian University of Würzburg, where he remained until his death, in spite of calls to the Universities of Heidelberg, Vienna, Berlin, Jena, Bonn, and Munich. He was nominated a Geheimrath, and was ennobled by the King of Bavaria. His election as Foreign Member of the Linnean Society dates from 1878, and of the Royal Society from 1888.

To give anything like an adequate account of Sachs' labours would be to write the history of plant-physiology from 1857 to 1885, after which time his active work, in consequence of ill-health, may be said to have ceased. Nothing more that a mere sketch of his scientific career will be attempted.

It appears from the 'Catalogue of Scientific Papers,' that Sachs' activity as an author dates from the year 1853, when he published three papers in the Bohemian journal 'Ziva,' two of which were

zoological (on the Crayfish and on *Dinotherium giganteum*) and one botanical ("Ueb. das Wachsthum der Pflanzen"), followed by a series of botanical papers, mostly morphological and systematic, which appeared in the same journal in the course of the years 1854–56. During this period two papers of his appeared in the 'Botanische Zeitung' (1855) on *Collema* and *Crucibulum* respectively; the former of these is of special interest inasmuch as in it Sachs expresses views which nearly anticipated the theory of the structure of Lichens propounded by Schwendener in 1868, and now almost universally accepted. But it is not until 1857 that we find him devoting himself especially to that department of botany with which his name will always be associated, when he commenced that series of physiological investigations which he carried on, with scarcely an interval, for more than a quarter of a century. The most important line of research which he struck out in what we may term his Prague-period, was that of investigating the metabolism of the plant by means of the micro-chemical examination of the tissues. Beginning with the paper "Ueb. einige neue microscopisch-chemische Reactions-methoden,"* it led on to the series of "Keimungsgeschichten," relating to a variety of plants (bean, grasses, date, onion, &c.), the publication of which extended over a number of years, and which may be fairly said to have laid the foundation of our knowledge of micro-chemical methods, as also of both the morphological and physiological details of the process of germination.

The period of his tenure of office at Tharandt is memorable for his resuscitation of the method of "water-culture," originally suggested by Duhamel,† and the application of it to the investigation of the fundamental problems of nutrition. It is to the introduction of this method that we owe whatever accurate knowledge we possess of the relative physiological importance of the various mineral constituents of the plant's food.

Of the many valuable contributions to plant-physiology which Sachs made whilst at Poppelsdorf (1861–67), perhaps the most fundamentally important are those which relate to the function of chlorophyll. Whilst Von Mohl and others had recognised the almost universal occurrence of starch-grains in the chloroplastids, it remained for Sachs to give the explanation of the fact. He ascertained that the formation of starch-grains in chloroplastids is dependent upon exposure to light; in other words, upon the conditions already known to be essential for the absorption and decomposition of carbon dioxide by the green parts of plants. Bringing these facts into their proper correlation, he arrived at the weighty conclusion that the

* 'Sitzber. d. k. Akad. d. Wiss. in Wien,' vol. 26, 1858.
† 'Physique des Arbres,' 1753.

starch-grains to be found in chloroplastids are the first visible pro-
duct of their assimilatory activity. This period of Sachs' life was
that in which he first made his mark as a writer of books. In 1865
appeared his ' Handbuch der Experimental-Physiologie der Pflanzen '
(as vol. 4 of the uncompleted ' Handbuch der Physiologischen Botanik,'
edited by Hofmeister); and although his well-known ' Lehrbuch der
Botanik '* was not actually published until 1868, the work fairly
belongs to the period under consideration. In neither of these books
do we find much indication of that literary ability which marks
his later works, no doubt because their structure rendered any
attempt at literary form impossible. They are both of them mines of
information, especially as regards the older botanical literature. But
they are much more than this. Though they may both be included
under the designation " text-book," unlike most text-books they con-
tain a large mass of original work on the part of the author. Thus,
they are not only learned, but also stimulating. It is not too much
to say that they have contributed very largely to the unprecedented
expansion of morphological and physiological research which has
taken place since their publication, a statement which is especially
true with regard to this country. They are generally admitted to be
the best works of the kind which had appeared up to that time, and
although they have now become somewhat antiquated, it is doubtful
if they have been excelled by any such works which have appeared
since. One feature in them deserves special notice, and that is the
manifestation, in the illustrations of the ' Lehrbuch ' in particular, of
the remarkable artistic faculty which Sachs possessed.

The preparation of the earlier editions of the ' Lehrbuch ' is no
doubt accountable for the relatively small number of original
memoirs which Sachs published during the years 1865–72, a period
which included his brief sojourn at Freiburg and his settlement at
Würzburg. But he had not been long in Würzburg before he
resumed his researches, the more important of which were published,
together with contributions by his colleagues and pupils, in the
well-known ' Arbeiten des Botanischen Instituts in Würzburg,' the
first number of which appeared in 1871. His papers in the first
volume of the ' Arbeiten ' (1871–74) all have reference to the
phenomena of growth, and include his remarkable investigations
into the periodicity of growth in length, which clearly established
the retarding influence of the highly refrangible rays of the spectrum.
The contents of the second volume (1878–82) are of very great
interest. We find here the last of his researches on growth, intro-
ducing the " clinostat," an important addition to the apparatus of

* An English translation of the third German edition was published by the
Oxford University Press in 1875; and another, from the 4th German edition
(1870), was published in 1882.

experimental physiology, to which Sachs had already so largely contributed. Then there are his classical papers on the structure of and the arrangement of cells in growing-points; and finally the papers in which he elaborated his "imbibition-theory" of the transpiration-current. Though it now seems probable that this theory is insufficient to explain the facts—the problem, however, still remaining unsolved in spite of many subsequent attempts at solution—yet these papers contain a great mass of important observations, the result of enormous labour and experimental skill, which must be taken into account by all who attempt investigation in this direction. This volume also contains the noteworthy papers on " Stoff und Form der Pflanzenorgane," an attempt at a material explanation of the differences distinguishing the chief members of the plant-body, as also the various metamorphosed forms of one and the same member. This attempted explanation, though it cannot be said to have met with general acceptance, has still its adherents, and is to some extent borne out by his subsequent observations, published in the third volume of the ' Arbeiten,' on the suppression of the development of flowers in plants grown in light which has passed through a solution of quinine, and has consequently lost the ultra-violet rays.

The third volume of the ' Arbeiten ' (1884–88) contains relatively little of Sachs' own work. The only paper of importance, in addition to the one just mentioned, is that entitled " Ein Beitrag zur Kenntniss der Ernährungsthätigkeit der Blätter," which is an exhaustive study of the assimilatory activity of the leaf, as also of the distribution of the products of this activity from the leaf to the rest of the plant.

With this third volume Sachs' work as an investigator may be said to have closed, though he subsequently published from time to time in ' Flora,' a series of " Physiologische Notizen " which, though slight, never failed to be suggestive and interesting, as his writings always were.[*]

Amongst the more important of his achievements whilst Professor at Würzburg must be reckoned his ' Geschichte der Botanik ' and his ' Vorlesungen ueber Pflanzenphysiologie.' In these works his great literary ability had scope to display itself. The style is lucid, with many brilliant passages; and the matter, widely different as it is in the two books, is handled with complete mastery in both. The ' Geschichte,'[†] which appeared in 1875, though by no means a bulky volume, presents a vivid picture, heightened here and there with touches of caustic criticism, of the gradual development of the science from the middle of the sixteenth century up to the year 1860. It is not only brilliant, but also learned, and its preparation must

[*] An edition of Sachs' collected papers was published in 1892–93.
[†] An English translation was published by the Oxford University Press in 1890.

have involved a vast amount of research into the older botanical works, not a few of which have been thus rescued from oblivion.

The 'Vorlesungen ueber Pflanzenphysiologie,'* which appeared in 1882 (second edition 1887), was prepared to take the place of the physiological sections of the 'Lehrbuch,' the preparation of a revised edition of the morphological and systematic portions being assigned to a former colleague, Professor Goebel.† The method of the 'Vorlesungen' is, however, altogether different from that of the 'Lehrbuch.' There is in it but little trace of that laborious compilation of facts from all sources which is the characteristic feature of the 'Lehrbuch.' On the contrary, the references are comparatively few. The book is, in fact, an exposition of the physiology of plants from his own standpoint; it is his confession of faith. In reading it one seems to hear Sachs himself speaking, an illusion which is to some extent justified by the fact that it was taken down from his dictation.

In closing this brief appreciation of his scientific work, it only remains to point out that there is scarcely a branch of botanical knowledge to which Sachs did not make some important and lasting addition.

As a lecturer Sachs was pre-eminent: he possessed great readiness of utterance, together with rare force and lucidity. His experimental lectures in particular will not easily be forgotten by those who have had the privilege of attending them. The eloquent speech, the pictorial illustration—generally by means of a stick of charcoal and large sheets of white paper—the manipulative dexterity, all combined to make these lectures fascinating and to rivet attention, even though their duration was never less than, and often exceeded, two hours. But, beyond all his other gifts, Sachs was endowed with a vigorous and stimulating personality, with the faculty of arousing in others the enthusiasm with which he himself was overflowing. In his best days at Würzburg, his laboratory was filled with students from all the countries of Europe. Among those who worked there are many well-known Continental botanists, such as Pfeffer, Goebel, de Vries, Prantl (who died a few years ago), Errera, Wortmann, Noll; and not a few Fellows of the Royal Society, I. B. Balfour, F. O. Bower, F. Darwin, W. Gardiner, D. H. Scott, H. M. Ward, and the present writer. Great as has been the direct influence of his own work upon the progress of botany, the indirect effect exerted through his pupils has been even greater.

Apart from his botanical knowledge, Sachs was a man of wide

* An English translation of this work was published by the Oxford University Press in 1887.

† An English translation of this book, with the title 'Outlines of Classification and Special Morphology,' was published by the Oxford University Press in 1887.

reading, with a special bent in the direction of philosophical studies which he found time to pursue in the intervals of his engrossing professional work. Though he spoke English imperfectly, he could read it with ease, and thus kept himself abreast of the advance of scientific and philosophic thought in this country. The writer well remembers that when, in 1877, he was working at Würzburg, Sachs was engaged with the writings of Herbert Spencer and of Lecky. Possessed, as he was, of a fund of humour, of a singularly acute intellect, and of a great store of information, Sachs was a brilliant talker. Altogether he was a remarkable and conspicuous figure, not among the botanists only, but among the men of science of his time; and in losing him the Royal Society has to deplore the loss of not the least distinguished of her Foreign Members, and of a worthy successor to Grew, Malpighi, Hales, and Knight.

<div align="right">S. H. V.</div>

SAMUEL HAUGHTON, who died on the 31st October, 1897, sprang from an old Quaker family, and although both of his parents had withdrawn from the Community of Friends, he spent his boyhood amidst Quaker surroundings. The early impressions which he then received remained permanent through life, and he retained deep-rooted within him many of the best of the Quaker principles.

He was born in Carlow on the 21st December, 1821, and was early sent to a large school which was kept in that town by the Rector of the parish. It was during his school life that his interest in natural science was awakened. He had the good fortune to come under the influence of Mr. Emerson, one of the masters of the school, a most gifted scholar, and a man endowed with a profound love of nature. With him as an associate young Haughton roamed over the surrounding country in search of specimens. The banks of the beautiful river which flows through his native town, the bog-land in the immediate vicinity, and the slopes of the neighbouring hills, were systematically explored, and botany and geology became his favourite recreations.

At the age of seventeen Haughton entered Trinity College, Dublin. He possessed in a remarkable degree the qualities which lead to success in college life, quickness of apprehension, a clear head, and a tenacious and ready memory. Indeed the distinction which he attained as a student was such that immediately after taking his degree, an unexpected vacancy having occurred, his friends induced him to enter as a candidate for Fellowship. With little more than six months' preparation, he succeeded at his first trial (1844) in that most formidable examination, a feat quite unprecedented in the history of the college. Set free thus early from the prolonged drudgery which is the usual preliminary of a Fellowship contest, Haughton had the rare good fortune to be able, untrammelled by

the fear of impending examinations, to follow out those lines of study and research which his natural bent of mind made him specially qualified to undertake.

He lived in the same house in college as the eminent mathematician McCullagh, and conceived the warmest regard and admiration for him. Very possibly it was due to this association that Haughton's earlier work was in the domain of physical science ; and at the age of twenty-six he obtained his first extra-collegiate distinction, viz., the award of the Cunningham Medal by the Royal Irish Academy for his memoir " On the Equilibrium and Motion of Solid and Fluid Bodies." Soon, however, he turned his attention to geology, and in 1851, on the removal of Professor Phillips to Oxford, he was elected University Professor in that subject. This chair he occupied for thirty years, and only resigned it on his appointment as one of the Senior Fellows of Trinity College in 1881.

Very early in his career, before he had entered college, Haughton had shown a strong inclination towards the profession of medicine. It was his boyish dream to prepare himself as a medical missionary, and to devote his life to missionary work in China. It was the old Quaker instinct working within him, an instinct which became the great ruling principle of his life, the desire to succour and help the weak in the great battle of life. It is fortunate for science and the cause of education that his early ambitions were not realised. Still, although his thoughts were for the time deflected into other channels, and his duties as a Fellow of the College and as Professor of Geology led him into a totally different field of work, his early yearnings were not completely eradicated. The natural course of events had drawn him away from medicine, but geology brought him back to it. He perceived that he could not properly treat of animal remains preserved in fossils without a knowledge of comparative anatomy, and the readiest means of obtaining this knowledge appeared to him to lie in the thorough study, in the first instance, of human anatomy. He was thus led to enter the Medical School, and consequently we find him, at the somewhat advanced age of thirty-eight, and already a Fellow of the Royal Society, already widely known as a mathematician and a geologist, undergoing all the drudgery attending a course of professional study. Well aware that there is no royal road to knowledge, he threw himself into the full routine of attendance on lectures and hospitals, and pursued his dissections and laboratory work as cheerfully as the youngest student in the school, and as assiduously as if he had to earn his bread by the practice of medicine. Although greatly burdened by other duties, he passed all the degree examinations at the prescribed periods, and finally graduated in medicine in 1862.

Thus introduced into the inner life of the School of Physic,

Dr. Haughton could not help seeing that it stood sadly in need of reform. Although the medical degrees of the University were at that time eagerly sought by young medical aspirants, the school of Trinity College was the last place where they cared to study. Immediately after his graduation, Dr. Haughton threw his whole energies into the improvement of the school, and to attain this end he was not slow to use the influence which he had deservedly acquired with the senior members of Trinity College. He was appointed Medical Registrar, and at once proceeded to work. To render the reform effectual it was necessary to grapple with many ancient abuses—a task from which he did not shrink. He was a man of unbounded courage and great pugnacity—to join in a fight was never unwelcome. But these instincts were kept in check by great kindliness of heart, and none of his former antagonists who may have survived him has cause to remember with pain any expression he ever used. The years which followed proved a somewhat stormy period in Dr. Haughton's life, and he had to adopt in certain instances measures which might appear to be harsh, but which were rendered absolutely necessary by the exigencies of the case. One of his leading characteristics was his absolute unselfishness, and it may be well to mention in connection with these controversies that no one could point to a single act of his which was dictated by self-interest. But he was stern and almost unforgiving with those who, by idleness or otherwise, did damage to the good name of the college he so sincerely loved. When Dr. Haughton took in hand the reform of the School of Physic it was one of the most insignificant of the schools which then existed in Dublin; now it takes the foremost rank, and this change, brought about in little more than thirty years, is largely, if not entirely, due to the sagacious and enlightened manner in which he guided its policy.

It was during the earlier years of his connexion with the medical school that he commenced a series of observations on the mechanical principles of muscular action. The results of these investigations appeared from time to time in the 'Proceedings of the Royal Society,' and of the 'Royal Irish Academy,' and ultimately took final shape in his book on 'Animal Mechanics,' which was published in 1873. This is probably Dr. Haughton's greatest work. For ten years he laboured at it, and during that time it was his daily practice to spend two hours in the dissecting room in the study of the comparative anatomy of the muscular system of vertebrates. What may be regarded as being the key-note of the work is struck in the following short extract from the preface. He says:—" I have met with numerous instances in the muscular mechanism of vertebrate animals of the application of the principle of least action in Nature; by which I mean that the work to be done is effected by means of the

existing arrangement of the muscles, bones, and joints with a less expenditure of force than would be possible under any other arrangement ; so that any alteration would be a positive disadvantage to the animal." In this—as indeed in all his writings—he takes up a most uncompromising attitude towards the theory of evolution or, as he expresses it, " the unproved hypothesis" of the descent of living organisms from "a supposed common ancestor." He would have none of it.

In 1878 Dr. Haughton retired from the post of Medical Registrar, and became the chairman of the Medical School Committee and University Representative on the General Medical Council.

With regard to his work as a member of the General Medical Council, Sir William Turner writes: " Dr. Haughton, as might naturally be expected, gave especial attention to matters connected with medical education. His speeches on the preliminary or entrance examination of intending medical students were characterised by shrewdness, both of thought and expression, and were enlivened by wit and humour. He held that the entrance examinations to the medical profession should be conducted by the national bodies engaged in general education, and he advocated the importance of mathematics as a mental training. His brightness and warmth of nature made him very popular with his colleagues, who, when his declining health made it necessary for him to resign his seat, expressed, through the President, their regret that he was no longer able to assist them in the discharge of their duties."

As a Governor of Sir Patrick Dunn's Hospital Dr. Haughton likewise did noble work. For thirty-four years he laboured in its behalf, and even during the failing years of his life his interest never slackened, and he rarely missed a Board meeting. The undaunted courage which he showed during the epidemic of cholera in 1866 is not likely to be forgotten. At that time there was most inadequate provision for the proper nursing of the cholera patients in Dublin, and as the disease spread, very naturally the entire nursing machinery broke down. Dr. Haughton called for volunteers from amongst the students and organised from them a nursing staff which did duty during the time that the epidemic lasted. In this work he drew no distinction between himself and the student members of the staff. He took his turn at nursing with the rest ; and, by the energetic and enthusiastic way in which he grappled with the difficulty, he did much to alleviate the suffering of the sick and panic-stricken poor in Dublin at a most trying and anxious time. The experience which Haughton passed through during the outbreak of cholera left on his mind an abiding sense of the value of bedside work. He was thus led to found medals for the encouragement of clinical work in Dunn's Hospital, and the last act of his life was, out of very scanty

savings, to provide for the making of these rewards more substantial and permanent.

The Government having thrown open to public competition commissions in the Artillery and Royal Engineers, Haughton was not deterred by his many other duties from organising, in conjunction with his friend Mr. Galbraith, classes for such students as were preparing for these services. The success of their teaching was so remarkable that the proportion of Irish candidates who obtained commissions was higher at that time than it has ever been since, and this success continued until the conditions of the competition were altered to the detriment of University candidates. In connexion with these classes he published with Mr. Galbraith a series of manuals which became text-books for the general use of the College.

No account of the part which Dr. Haughton has played as a prominent figure in Irish life would be complete without reference to his long and intimate connexion with the Royal Zoological Society of Ireland. In 1860 he became a member of its Council, and in 1864 he was elected its Honorary Secretary. For twenty-one years he filled this office, and he only resigned it to assume the duties of President, which he discharged for five additional years. During a large part of the time in which he acted as Secretary the Society was in a very struggling condition, and it passed through more than one financial crisis which threatened to swamp it; indeed it is very probable that had there been a less courageous and able man at the helm the Society would have been submerged altogether. But Haughton never lost heart, and the Society owes its present secure position in a great measure to the plucky fight which he then fought. He enjoyed extremely telling how when the bank on one occasion threatened to foreclose the Society's account, he had met the difficulty by offering to deposit a ferocious Bengal tiger as security for the debt.

He had a passionate fondness for animals. He was never seen in Dublin without his dog; and on his death bed, when lying speechless, he endeavoured with the little power which still remained in his left arm to acknowledge the advances of his little Scotch terrier.

Some of the happiest hours of his life were spent in the Zoological Gardens. A distinctive feature of the social life in Dublin consists in the weekly "Zoo breakfasts." On Saturday mornings the members of Council meet in the Gardens and take breakfast together before proceeding to business. It was at these meetings that Haughton appeared at his very best. Surrounded by friends of long standing, all of whom had the greatest admiration and affection for him, he was wont to give full scope to his bright and joyous nature.

Among Haughton's many talents perhaps the greatest was his

talent for acquiring friends. As we have said, he was singularly unselfish, for if his friends had any fault to find in him it was his little care of his own material interests: he was perfectly transparent and sincere, and absolutely loyal. Thus he kept all the friends he made, and no one was more successful in gaining new ones. He was one of the most charming of companions, overflowing with wit and humour, ready to take a lively part in any discussion, and able from a well-stored memory to relate many results of a much varied experience. And those who were attracted by his social qualities found, as they came to know him better, that he possessed those sterling qualities on which the solid foundations of friendship can be laid. He used to be a regular attendant at the meetings of the British Association, the chief use of which is the bringing of men of kindred pursuits together. There he formed valuable friendships with scientific men, of whom it may be enough to name Tyndall, who came from the same part of Ireland, and Huxley, with whom, though they were widely apart on their theological views, he always maintained a cordial and intimate friendship. His medical friends outside Ireland are too numerous to be mentioned, but he used frequently to speak of Acland of Oxford, Sir William Turner, Sir John Simon, and Sir Richard Quain. It is needless to say how his loss is felt by those with whom he was in the habit of daily working, and it may in truth be said that no man has left a wider circle of sorrowing friends.

He received many honours during his life. Only a few of these need be alluded to here. He was elected a Fellow of the Royal Society in 1858. He received the degree of D.C.L. (Hon. causâ) of Oxford in 1868; the degree of LL.D. (Hon. causâ) of Cambridge in 1880; the degree of LL.D. (Hon. causâ) of Edinburgh in 1884; the degree of M.D. (Hon. causâ) of Bologna in 1888.

It is impossible to give in the space at our disposal anything like a proper conception of the published work of Dr. Haughton. Few men had a greater multiplicity of interests, and although it is probable that if he had been more of a specialist he would have left behind him a larger amount of work of permanent value on some one subject, yet his many-sidedness pre-eminently fitted him for the place he filled in the government of a large educational institution. In the Royal Society Catalogue 173 memoirs and papers are entered under his name, and this only takes his work down to 1883. The majority of these were published in the 'Proceedings of the Royal Irish Academy,' of which he was a member for fifty-two years, and of which he lived to become the President.

Some idea of his amazing versatility may be gained by a glance through the titles of his papers as they are given in the Royal Society's Catalogues. The following is a small selection from these :—

"An Account of Experiments made on a new Friction Sledge for
    stopping Railway Trains."
" Physiological Experiments on Nicotine and Strychnia."
" On the Sea-Louse of the Baltic."
" On the Reflexion of Polarised Light from Polished Surfaces—
    transparent and metallic."
"Account of Experiments to Determine the Velocity of Rifle
    Bullets."
" On the Muscular Anatomy of the Leg of the Crocodile."
" On Hanging, considered from a mechanical and physiological
    Point of View."
" On Geological Climates."
" On the normal Constants of healthy Urine in Man."
" On the Tides and tidal Currents of the Irish Sea, &c., &c."
" On Slaty Cleavage and the Distortion of Fossils."

Dr. Haughton's contributions to physical science were principally
in the subjects of elasticity, the theory of light, solar radiation, and
the tides.

Fond of controversy, he plunged into the discussion which raged
in the middle of the present century on the nature of transparent
media. He very early came to the conclusion (1849) that it was
only by a study of the facts of reflection and refraction that the
question could be decided, and with this end in view he made a
great number of laborious observations on the refraction of polarised
light from many substances. The results he obtained remain as a
monument to his industry and a permanent contribution to science,
although the controversy as to the nature of transparent media has
drifted into new channels, owing to the development of the electro-
magnetic theory of light.

His work on solar radiations was undertaken in connexion with the
geological question as to the age of the earth, the causes of the
glacial epoch, and of geological climates. By the aid of very
laborious calculations he considered the effects produced on terres-
trial climates by changes in the distribution of land, by alteration in
the temperature of the sun, and by the quantity of carbonic acid and
aqueous vapour in the air.

His observations on the tides were originally undertaken with the
view of rendering the navigation of the Irish Sea and the English
Channel safer to outward and homeward bound ships. He after-
wards became a recognised authority on tides, and was not only
consulted by Arctic explorers, but was entrusted with valuable
records obtained during Arctic voyages for the purpose of report.

Dr. Haughton likewise devoted much of his time to chemical
problems, and in the later years of his life some of the higher

mathematical conceptions of chemistry, which he himself called " Newtonian chemistry," greatly engrossed his attention. He endeavoured to develop the consequences of a theory of chemical combination depending on the assumptions that in chemical actions energy was conserved as well as a quantity analogous to areas. Our knowledge of the dynamics of molecules is hardly great enough to criticise such a theory effectively.

Dr. Haughton's work in the field of geology includes contributions upon subjects of a very diverse nature. He has written extensively on mineralogy, petrography, physical geography, and physical geology.

Mineralogy first claimed his attention, and in 1853 he began to publish a series of papers on Irish minerals. One of the last of these dealt exhaustively with the fine meteorite which fell at Dundrum in co. Tipperary, and which through his instrumentality was presented by Lord Hawarden to the Museum of Trinity College.

His petrographical communications are still more numerous and important. His work on the Irish granites is singularly complete, and was commenced in 1856, at a time when the use of the petrographical microscope had not been revived by Sorby. When this is taken into account we cannot help being struck by the results obtained by Haughton.

A highly interesting observation is contained in his papers on the Trap Dykes of the district of Mourne and on the Carlingford Granite. He detects a chemical reaction between the intruding granite and the limestone, and points out how the granite is thereby altered to what may be called a Syenite.

On the subject of physical geology Haughton contributed many papers to the 'Proceedings of the Royal Society' and elsewhere. The first of the series relates to the effect on the distribution of climate in geological time from the shifting of the earth's axis, due to continental upheaval. Haughton arrived at the conclusion—which is in agreement with the views of Professor G. Darwin—that the effect would be insignificant. In a paper which appeared in 'Nature,' Haughton estimated the whole duration of geological time as 200 million years. This opinion he based on the probable rate of formation of stratified rock. He assumed these to possess a thickness of 177,200 feet. He likewise investigated the question of geological climate in connexion with Rossetti's 'Law of Cooling,' and arrived at the conclusion that the secular cooling of the sun has been the chief factor in changes of geological climate.

In 1858 he published in the 'Philosophical Transactions' his important work on the joint-planes of the Old Red Sandstone of co. Waterford, and with this also may be associated his observations on the remarkable uniformity in magnetic bearing of the joint-planes

of these rocks with the joint-planes of Cornwall granite, Donegal granite, the carboniferous limestone of co. Fermanagh, and the granite and slate of Mourne. Daubrée has likewise expended infinite pains on the subject of joint-planes.

Haughton's book entitled 'Lectures on Physical Geography' gives some idea of his marvellous grasp of facts, and of his many-sidedness. His 'Manual of Geology' is also in many respects a remarkable book. The chapter on " Symmetry " is specially characteristic of the writer. Those who read the closing chapter of that book, and who cannot accept the opinions he expresses regarding Darwinism, will at least respect the deep and sincere feeling which caused him to reject the Darwinian philosophy as being opposed to his religious views.

<div style="text-align: right">D. J. C.</div>

WILLIAM FRANCIS DRUMMOND JERVOIS was born at Cowes on the 10th September, 1821. In February, 1837, he joined the Royal Military Academy, Woolwich, and two years later obtained his commission in the Royal Engineers. After passing through the School of Military Engineering at Chatham, over which Sir Charles Pasley then presided, he sailed for the Cape of Good Hope. Here he was soon actively employed. When only twenty-one he was appointed Brigade-Major to a force sent to the Orange River to control the movements of the Boers; and in 1846-7 he took part in an expedition against the Kaffirs. On the latter occasion he made, under circumstances of considerable difficulty, a military sketch of British Kaffraria which was of great use in subsequent wars. Thirty years later it was the only map of the district, possessing any pretensions to accuracy, which the general commanding could find for his guidance. In 1848 Captain Jervois returned to England with a special recommendation from the Governor to Lord Raglan as an active, able officer who could "afford every information on all military and geographical points" connected with the colony.

The Duke of Wellington had always held strong views with regard to the military importance of Alderney, and, in 1852, it was decided to protect the harbour of refuge, then in course of construction, by strong fortifications. This duty was entrusted to Captain Jervois, who designed the works and superintended their construction. Sir W. Jervois maintained to the last that Alderney was still a place of importance in the defence of England, but other views prevailed, and the forts he constructed are now in ruins.

In 1855 Major Jervois was transferred to London and served on Lord Monk's Committee on Barrack Accommodation. The following year he was appointed Assistant Inspector-General of Fortifications, and with characteristic energy at once took up the question of the defence of our dockyards. He studied the ground at Portsmouth

and Plymouth, prepared projects for occupying particular lines of ground by forts, and, with a staff of specially selected officers, designed the works to be constructed.

The movement for the protection of our dockyards and naval arsenals originated in the celebrated letter of the Duke of Wellington and in a Memorandum prepared in 1847 by Lord Palmerston with the assistance of Sir John Burgoyne. In the latter paper it was pointed out that it was possible for France, under certain conditions, to land a force which, amongst other operations, might destroy our dockyards, and so paralyse our naval resources for years. Public opinion was at the time opposed to the movement, but, after the Crimean War, a change took place, and some progress was made, as already mentioned, in designing works. It was not, however, until 1858, when the Government received private information that the French were secretly making preparations for war, and were obliged to take measures of precaution, that the importance of the question forced itself upon public attention. In 1859 Lord Palmerston, who was a strong supporter of the view that it was necessary to secure our naval bases against any hostile enterprise, again became Prime Minister. A Royal Commission on the National Defences was appointed, with Major Jervois as secretary, and, in 1860, it reported to Parliament. After much discussion a resolution was carried in the House of Commons to the effect that the recommendations of the Commission should be carried out as rapidly as possible, and that the cost should be met by a loan.

Major Jervois, who had practically guided the work of the Commission, and the preparation of the Report, was now entrusted with the task of fortifying the naval bases and arsenals and the principal harbours and coasting stations of the United Kingdom, the Colonies, and dependencies. On this duty he was employed until 1875 when the accounts of the great loan for fortifications were finally wound up with a saving of more than £200,000. The nineteen years spent at the War Office formed the most important period of Sir W. Jervois' life. During the whole of it he acted as secretary to the Committee on the Defence of the Empire, and rendered very valuable services to his country. The fortifications, chiefly from a mistaken view of their object, have been much criticised. They were projected and carried out in accordance with the strong recommendations of men of the largest experience in actual war, and were intended to render the bases from which the navy worked secure. The great works themselves will always be a monument to the ability and energy of Sir W. Jervois. The difficulties that had to be encountered and overcome were very great. When the work was commenced rifled artillery was in its infancy; armour plating was in the experimental stage; torpedoes and submarine mines had made no progress; and

high explosives for mines and shells were still in the distant future. There was no delay, however, in adapting the works to the changes rendered necessary by these developments. The lines of defence were laid out further from the dockyards to meet the increased range of artillery; arrangements were made by which the armour plating of the forts could be strengthened to meet any further development of artillery; and a committee was appointed to consider how submarine mines and torpedoes should be utilised to strengthen the defence of ports and harbours.

The works during and after execution were sharply criticised, and they were defended with energy and success by Colonel Jervois before committees and in papers read at the Royal Institution and the Royal United Service Institution. Fortifications constructed from twenty-five to thirty years ago must obviously need modification in some points to enable them to meet later developments in the art of attack. But, on the whole, it may be said that the works for which Sir W. Jervois and his associates are responsible constitute a solid and enduring contribution to the defences of the country, and that for many years to come they will fulfil the object for which they were erected. Amongst the greatest of those works are the fortifications for the protection of Portsmouth, Plymouth, Portland, Pembroke, Cork, the Thames, and the Medway.

In 1863, 1864, and 1865 Colonel Jervois went on three separate missions to advise on the defence of Canada; in 1863 and 1869 he was sent to inspect the works at Halifax and Bermuda, and in 1865 and 1866 to prepare projects for strengthening the fortifications at Malta and Gibraltar. In 1871–72 he was employed by the Government of India to inspect and report on the defences of Aden, Perim, Bombay, and the Hûghli, and he also visited British Burma, and reported on the defences of Rangûn and Mulmein.

In 1874 Sir W. Jervois, who had been made a C.B. in 1863, was created K.C.M.G. in recognition of his services to Canada. In 1875 he left the War Office and was appointed Governor of the Straits Settlements. Soon after his arrival at Singapore he found himself confronted by the necessity of making war on the Malay States of Perak and Sungei Ujong, to punish them for the treacherous murder of Mr. Birch, the British Resident. By his promptitude in concentrating a strong force at the seat of war, the operations were soon brought to a successful conclusion, and he then elaborated plans for the government of the protected Malay States. In 1877 he was sent to Australia to advise the Australasian Governments on matters of defence, and to prepare schemes for the protection of their ports. Whilst employed on this duty he was appointed Governor of South Australia, and, after completing a term of six years (1877–83), was made Governor of New Zealand for a like term (1883–89). In

these two colonies he showed himself to be as good a constitutional governor, working with ministers responsible to a Parliamentary majority, as he had proved himself to be a good autocrat in the Straits Settlements. From 1877 to 1889 he was adviser to the Governments of all the Australasian colonies on questions connected with the defence of their harbours and coasts, and during this period he placed the defences in a much better position to resist any incursion by the ships of hostile Powers than they were on his arrival.

In 1889 Sir W. Jervois, who had been made a G.C.M.G. in 1878, returned to England, and in the following year was appointed a member of Mr. Stanhope's Commission on Military Defences (1890–91). In 1888 he was elected a F.R.S. In 1892 he wrote an article in the 'Nineteenth Century' magazine advocating that the coast defences should be placed in the hands of the navy—which attracted some attention at the time. But most of the years of his retirement were passed quietly in the country, until an unfortunate carriage accident ended his life on the 17th August, 1897.

C. W. W.

WILLIAM ARCHER was the eldest son of the Rev. Richard Archer, vicar of Clonduff, Rathfriland, co. Down. He was born on May 6, 1827. His two younger brothers were educated at Trinity College, Dublin, and were afterwards in the Government service, but no particulars of his own early education are available. The significant fact of his life was the foundation in 1857 of the Dublin Microscopical Club, of which for a long period Archer was secretary, and to a large extent the moving spirit. The club was started by a small group of students, who were drawn to natural history studies by the teaching of Allman and Harvey, the two distinguished men who successively occupied the chair of botany at Trinity College, Dublin. The club, which still exists, was limited to twelve members, and probably no society so small has ever accomplished so much important scientific work. Each member from the first took up some special line, and Archer devoted himself to the investigation of the Protozoa and microscopic algæ of the moor-pools of Ireland.

In this fascinating field of research, the richness of which can perhaps hardly be paralleled in any other country, Archer laboured for the rest of his life. He devised a simple but effective method of collecting and of preserving for future examination his collected material. As might be expected a long experience gave him extraordinary dexterity in the work. Nothing could be more interesting than to accompany him on one of his excursions. He knew how to find his way through the bogs and instinctively selected the by no means obvious spots where the best harvest was to be found.

A striking instance of his extraordinary skill as a collector was his

detection in 1863 in a small rock-pool, no larger than a wash-hand basin, on Bray Head, of the beautiful Volvocinacea, *Stephanosphœra pluvialis*, Cohn, then only known from six isolated localities ranging from Scotland to Austria. He described for the first time an amœboid state of the constitnent cells.

When the writer of this notice went to reside in Ireland in 1870, the parting advice of a friend was to seek out and make the acquaintance of Archer. Under his guidance the treasures were revealed of a field of nature as fascinating as it was novel. Nothing can be imagined more entrancing than the work under Archer's guidance of examining with the microscope the results of a day's gatherings.

In the course of the long period which Archer devoted to their study he acquired a knowledge of the minute fresh-water organisms of Ireland which was certainly unequalled amongst British naturalists, and perhaps not surpassed for any other country. But he did not content himself with the mere identification and cataloguing of forms. He was constantly observant of their biological significance, and to follow the work of others in this respect, he made himself acquainted with the principal European, and especially Scandinavian languages. The chief of his detailed observations were communicated to the Dublin Microscopical Club, and are to be found in its minutes, which from 1864 were published in the 'Quarterly Journal of Microscopical Science.'

More extended studies were given in a series of separate papers published in various scientific journals. Of these the titles of fifty-nine are contained in the Royal Society's 'Catalogue.' He was fastidious in recording anything which he had not worked out to his complete satisfaction, and much of the results of his laborious research is doubtless lost. His most considerable contribution to algology is the revision of the Desmideæ in the second edition of Pritchard's 'Infusoria.'

It is, however, probably to his work amongst the Protozoa that Archer will owe his ultimate place in science. It was his good fortune to discover in 1868 *Chlamydomyxa labyrinthuloides*, one of the most remarkable and enigmatical of all known microscopic organisms. In 1867 Cienkowski had described, from the harbour of Odessa, Labyrinthula, the only other with which Chlamydomyxa can be compared. The two together form Lankester's class Labyrinthulidea; but, though both produce a protoplasmic network, Chlamydomyxa, unlike Labyrinthula, possesses a laminated cellulose shell, within which it is most usually found inclosed. As Lankester points out, it has obvious affinities with the Mycetozoa, but its ultimate place in classification is a problem which still awaits the result of further investigation. Archer's admirable research, the result of many years

observation, was, owing to his peculiar modesty, not extracted from him without reluctance; but it immediately procured his election into the Royal Society. It had been preceded, on its publication in 1875, by other papers scarcely less important, in which he established the Chlamydophora, a new order of Heliozoa, and described many other new genera, of which Rhaphidiophrys is one of the most remarkable. He trained himself to be an admirable draughtsman, and the beautiful illustrations to his papers fall somewhat short of the delicacy of the original drawings.

Archer was always a man of small means, and the necessary occupations of his earlier life were never very congenial to him. In 1872 the Marquis of Ripon, then Lord President of the Council, entirely unsolicited, offered Archer, who was personally unknown to him, the Professorship of Botany in the Royal College of Science for Ireland. Archer, with characteristic conscientiousness, shrank from the responsibilities of a teaching chair. Four years later his friends were more successful in getting him appointed Librarian of the New National Library, a post for which his careful and business habits admirably fitted him. He threw himself into the work of organising the library with characteristic determination, and no doubt impaired the strength of a constitution which was never robust. Unfortunately his duties largely withdrew him from scientific work. He retired on a pension in 1895, and died August 14 of the present year. He never married.

Apart from the scientific enthusiasm which dominated his character, Archer had a singular charm of manner. A gentleness and refinement of disposition, almost feminine, made him impossible to quarrel with, and he was one of those fortunate people who go through life without making an enemy. There was no want of robustness, however, about his scientific insight; but a quaint sense of humour would always parry a contentious criticism. A few words may be quoted from the notice by his friend, Mr. Frazer, in the October number of the 'Irish Naturalist' as to the regard with which he was held as a public servant :—" He was, as head of a great library, eminently successful in discharging his duties, and securing the esteem of his subordinates and of the public at large ; those especially who profited by his assistance in forwarding their literary researches will gratefully acknowledge their indebtedness to his patient and untiring desires to meet their wishes and advance their interests."

Archer filled the office of Secretary for Foreign Correspondence to the Royal Irish Academy from 1875 to 1880, and in 1879 was the recipient of its Cunningham Gold Medal.

W. T. T. D.

Louis Pasteur was born at Dôle, on the 27th of December, 1822, but his childhood was passed in Arbois, whither his family removed when he was an infant of two years old. His parents were in humble circumstances, his father being a hard-working tanner; that he was, however, a man of character and stern experience is shown by the fact that he had not only fought in the legions of the First Empire, but had been decorated on the field of battle by Napoleon, and bore the title of *Chevalier de la Légion d'Honneur*.

The home at Arbois appears to have been one of those establishments which revolve round the children, and the greatest sacrifices were made by the parents to secure the best educational advantages for the son. Pasteur, as a boy, however, showed but little inclination to work at books, preferring to play the truant and spend his time in following his favourite pastime of fishing or sketching portraits of his companions and neighbours. As he, however, grew older and began to realise the strain upon the resources of his parents which his education entailed, he, with that energy and determination which characterised all his actions throughout life, put away his fishing tackle and locked up his cherished pastels so as to place himself out of the reach of temptation and set to work. From that day onwards Pasteur may be said to have hardly ever paused in the pursuit of those Herculean labours which his genius throughout his life supplied in such rapid succession for his indomitable energy to perform.

The college at Arbois having no chair of philosophy, Pasteur went to Besançon, where he graduated Bachelier ès Lettres, and was appointed "maître répétiteur," in the College.

Pasteur's interest in chemical science had already commenced at this time, and his eagerness to acquire knowledge resulted in his overwhelming his teachers with questions, and we are told how one of these, the venerable Professor Darlay, was so embarrassed by his eager inquiries that he was reduced to telling young Pasteur that it was for him to interrogate his pupils, and not for them to catechise him before all his scholars! Pasteur, however, was not to be discouraged by treatment of this kind, and he went for private assistance in his studies to a pharmacist who enjoyed considerable local reputation through being the author of a paper which had been considered worthy of publication in the 'Annales de Chimie et de Physique.'

His old schoolmaster, at Arbois, had often urged upon Pasteur "pensez à la grande École Normale," and perhaps this encouragement led him to present himself for the entrance examination at this Institution. He obtained the fourteenth place, a position which so dissatisfied him that he withdrew, and going up again later on, in October, 1843, he was rewarded for his energy and perseverance by being placed fourth in order of merit.

As a student at the Ecole Normale, Pasteur enjoyed the privilege of attending the lectures both of Balard, at the École Normale, and of Dumas, at the Sorbonne. His energy was boundless, not even on Sundays did he rest from his chemical studies, and on one of these days of rest he actually succeeded in accomplishing the remarkable feat of preparing no less than 60 grams of phosphorus from bones, the operation lasting from four in the morning to nine o'clock at night.

But although Pasteur owed so much to each of those great masters of chemical science, Balard and Dumas, it was a junior man, M. Delafosse, who really directed the course which his researches should take. A former pupil and assistant of the renowned Haüy, M. Delafosse turned his pupil's attention to the study of crystals and to problems of molecular physics, in which domain Pasteur's first laurels were subsequently won.

On completing his curriculum at the École Normale, Pasteur was retained as assistant by Balard, and having now the opportunity of carrying on research he determined to perfect himself in crystallography, and set to work to repeat a very complete investigation made by M. de la Provostaye, on the crystalline form of the tartrates. It was soon evident, however, that mere repetition and confirmation were not Pasteur's strong points, for although a comparative novice at the kind of work in question, he was able to see what had escaped the observation of his skilled predecessor in this field. Thus, both on the crystals of tartaric acid itself, as well as on those of its salts, he at once found small facets which had not hitherto been described.

The presence of such facets on quartz crystals had not escaped the attention of Haüy, who indeed had further divided such quartz crystals into left and right-handed quartz, according to the side on which these facets were developed. Biot, again, in his extended investigations on polarisation, had found that some specimens of quartz turn the plane of polarisation to the right and some to the left, whilst Sir John Herschel, in 1820, suggested that the two phenomena were connected, and that the left-handed quartz crystal rotated the plane of polarisation in one direction, the right-handed in the other. Experiments showed that this was actually the case. These remarkable relations appear to have made a profound impression on young Pasteur, and so deeply imbued was he with the idea that polarimetric effect must be associated with crystalline form, that the appearance of these hemihedral faces (as these facets are technically called) seemed to him of the very highest importance, and to deserve the most careful study. To this end he prepared no less than nineteen different tartrates, and found that all exhibited hemihedral faces. Pursuing his minute examinations of these crystals, he found that whilst the crystals of the inactive tartaric

acid which were destitute of these little surfaces were symmetrical, the crystals of the optically active tartaric acid were unsymmetrical, or dissymmetric, as he called it. Now, to the symmetric character of the crystals of the one tartaric acid, generally known as paratartaric or racemic acid, he attributed the inactivity of this tartaric acid to polarised light; whilst with the dissymmetric character of the crystals of the other tartaric acid he connected its action on the polarised beam. In studying these apparently trifling details, Pasteur found that by crystallising the inactive tartaric acid in a particular way, by preparing the sodium ammonium salt and crystallising this, he obtained two different kinds of crystals—the one set being identical with those of the active tartaric acid already known, whilst the other set were the mirror images of these and had never been seen by the eye of man before. The young philosopher at once drew the conclusion that if the dissymmetry of the known tartaric acid caused it to turn the plane of polarisation to the right, the dissymmetry of this new tartaric acid should turn it to the left.

With infinite pains Pasteur picked out from the mixture the individual crystals belonging to each of the two types, and arranged them in two heaps. Each of these heaps of crystals was then separately dissolved in water, and the two solutions submitted to polarised light. In accordance with his anticipation, whilst the solution of the crystals of the known form was found to turn the plane of polarisation to the right, the solution of the new crystals, the mirror images of the old, was found to turn the plane through precisely the same angle to the left.

Of such moment did this discovery appear to Pasteur that, rushing from the laboratory in a fever of excitement, and meeting M. Bertrand in the corridor he embraced him, exclaiming, overcome with emotion : " Je viens de faire une grande découverte ;" and such indeed it was; but it is related how, whilst producing a great sensation in scientific circles, it was received with no little scepticism by the Academy of Sciences who instructed Biot to report upon it. Pasteur has himself described the sceptical and almost suspicious attitude adopted by this great investigator towards the work of this enthusiastic novice in the regions of scientific research, and how, as step by step Biot verified the accuracy of Pasteur's observations, he became more and more excited until, finally, when he found that the solution of the new crystals, as Pasteur had affirmed, exhibited a strong lævo-rotation in the polarimeter, he seized him by the hand, exclaiming with visible emotion, "Mon enfant, j'ai tant aimé les sciences dans ma vie que cela me fait battre le cœur."

Pasteur proceeded to point out that the differences in optical properties and in crystalline form exhibited by these two oppositely active tartaric acids were doubtless dependent on the two molecules

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having a different arrangement of their constituent atoms, the arrangement in each case being dissymmetric, and clearly indicated that whatever the dissymmetry of the one tartaric acid might consist in, it must be related to the dissymmetry of the other tartaric acid in the same sort of way as the dissymmetry of the left hand is related to the dissymmetry of the right hand. At the time, however, organic chemistry was not sufficiently advanced to make any immediate use of these speculations, but the philosophical reflections in which he indulges show how, so long ago, he had completely foreshadowed and grasped the scope of that important branch of chemical science now known as *Stereochemistry*.

But if we are impressed with the sagacity and suggestiveness of Pasteur's theoretical speculations, we are filled with even still greater admiration on again turning to his experimental work. The field of investigation which he exposed to view by his discovery of the relationship between the racemic and tartaric acids appears as limitless as the prairie which is bounded only by the horizon, and nothing can testify more eloquently to the experimental genius of Pasteur than the circumstance that already during the comparatively short period of time in which he himself was pursuing its cultivation, he succeeded in determining the exact methods by means of which it can be exploited. No new methods have been devised, no substantial modifications or improvements have been introduced, although many hands of divers nationalities have since been busily engaged in tilling the estate which he himself was constrained to abandon now nearly thirty years ago.

Pasteur's academic career was now assured, and the close of the year 1848 finds him Professor of Physics at the Lycée of Dijon, whilst three months after his installation there he was nominated Deputy Professor of Chemistry at the University of Strassburg, becoming full professor in 1852. His removal to Strassburg had another significance, quite apart from the greater scope which it afforded him for carrying on his scientific work, for here he met his future wife, the daughter of M. Laurent, Rector of the Strassburg Academy. Their marriage, which took place in 1860, was a singularly happy one, for Madame Pasteur, as Dr. Roux has said, became not only "une compagne incomparable," but Pasteur's "meilleur collaborateur."

During the five years he resided in Alsace, Pasteur devoted himself almost exclusively to the systematic investigation of asymmetric compounds. Associated with this period of his life we find those important and now classical researches on the conversion of right-handed tartaric acid into inactive tartaric acid (racemic acid) on the one hand, and into a new form of inactive tartaric acid (mesotartaric acid) on the other, his discovery of the method of splitting up

racemic acid into its component dextro- and lævo-tartaric acids by means of optically active bases, and his famous refutation of Dessaignes' reputed conversion of fumaric and maleïc acids into aspartic acid, identical with that hitherto only obtained from asparagine.

Pasteur had himself studied these various bodies before the publication of Dessaignes' memoir, and he had found that whilst fumaric and maleïc acids were not dissymmetric and were destitute of all optical activity, aspartic acid, on the other hand, like asparagine, from which it is derived, was endowed with molecular dissymmetry, and was active towards polarised light.

If Dessaignes' facts were correct, they would mean that he had accomplished what Pasteur firmly believed to be impossible—the preparation by artificial chemical means of an optically active molecule from an inactive one. Pasteur, with his usual restless energy, determined at once to set this doubt at rest, and hurrying to Vendôme he obtained from Dessaignes a specimen of his artificial aspartic acid. On returning to his laboratory, Pasteur examined it with the minutest care, and found that, in spite of its great resemblance to the acid derived from asparagine, it differed in a very important particular, inasmuch as it was entirely devoid of the action on polarised light which characterised the former, and he had no difficulty in showing that Dessaignes' acid was not identical with the natural aspartic acid, but only its inactive isomeride.

So far Pasteur had kept strictly to the domain of pure chemistry and molecular physics, and his attention was entirely absorbed by problems which, while of profound theoretical interest, gave no indication of the direction which his future labours would take, and to the pursuit of which his whole life was subsequently to be devoted.

It was an incident, trifling in itself, which first suggested to Pasteur the application of fermentation processes to the study of chemical substances. His attention having been called to a chance observation made by a German firm of manufacturing chemists, that solutions of impure commercial tartrate of lime fermented when left in warm weather in contact with organic matters, he determined at once to utilise this fact and induce, if possible, fermentation in a solution of ordinary right-handed tartaric acid. Dissolving a salt of this acid, he added to the solution a small quantity of albumen with the result that fermentation ensued and the liquid, originally clear, became gradually turbid, a phenomenon which Pasteur found was due to the presence of small living cells, upon which he subsequently showed the process of fermentation to be dependent. This method he also applied to solutions of the paratartrate (racemate) with the same results. On examining the solutions, after fermentation, with the polarimeter, the most profound difference was found

to exist between them. In the case of the paratartrate (racemate), the liquid, originally inactive, exhibited, as the fermentation proceeded, a gradually stronger and stronger deviation of the plane of polarisation to the left until the maximum was reached and the fermentation ceased. It was then found that during the process of fermentation the right-handed acid had been consumed, leaving the left-handed acid alone master of the field; and thus, freed from the constraining influence of its right-handed brother, was able to assert itself and exhibit, for the first time, its left-handed rotatory power. Thus whilst right-handed and left-handed tartaric acids are chemically identical, and are distinguishable only by their crystalline form and opposite action on polarised light, they are, nevertheless, utterly different from a physiological point of view; for the right-handed tartaric acid is alone taken up and transformed by the fermentative bacteria which refuse to have anything to do with the left-handed tartaric acid. Thus the apparently trivial difference in the arrangement of the atoms in space in the case of these two tartaric acids makes an overwhelming difference in their physiological character. A couple of years later, in 1856, the Royal Society conferred the Rumford Medal on Pasteur "for his discovery of the nature of racemic acid and its relations to polarised light."

A new chapter in Pasteur's life opens with the year 1854, when, at the age of 32, he was nominated the first Dean of the Faculty of Sciences which had just been created at Lille. In this capacity, at once realising that the work of his department should, to some extent, be brought into touch with one of the leading industries of the district—the manufacture of alcohol from beetroot and grain—he offered courses of lectures on fermentation, and with his characteristic energy threw himself into the serious study of his subject.

It is impossible here to pass even in the briefest review the history of the progress made in the knowledge of fermentation before the subject was attacked by Pasteur; suffice it to say that at this time fermentation processes were not generally regarded as vital phenomena at all. The dominant opinion concerning them was that enunciated by Liebig, who viewed the classical transformation of sugar into alcohol as a purely chemical process, depending not on the living yeast cells which the microscope had revealed, but on the dead yeast undergoing *post-mortem* decomposition:

"Beer yeast, and in general all animal and vegetable matters in putrefaction, impart to other bodies the state of decomposition in which they are themselves. The movement, which by the disturbed equilibrium is impressed on their own elements, is communicated also to the elements of bodies in contact with them."

Pasteur had been indirectly brought in contact with fermentation phenomena in the course of his researches on asymmetry, for amongst

the optically active compounds known at the time was the amyl alcohol, which is obtained as a by-product in a number of fermentations. In 1857 he brought before the scientific world the result of his researches on the lactic fermentation, the first of that series of masterly investigations which he was to pursue during the next twenty years. In 1860 this was followed by a paper on the alcoholic fermentation.

In the lactic fermentation, Pasteur noticed that a greyish solid material was deposited, and that the quantity of this increased during the process. On examining some of this substance under the microscope, he found that it consisted of very minute corpuscles, quite different from the yeast cells observed in the alcoholic fermentation, but which he felt convinced must be of analogous nature. Taking a trace of this grey material, he introduced it into an artificial solution of sugar, to which he had added some decoction of yeast and chalk, and soon he had the intense satisfaction of witnessing the lactic fermentation in full activity in this liquid. From this fermenting liquid he transferred again a minute trace into another similar solution of sugar, and so on, invariably obtaining the same fermentation, invariably finding also the same corpuscles in the deposit.

In order to meet the objection which he conceived might be raised by Liebig and his supporters that the fermentative change in the sugar was due to the decomposition of the albuminoids present in the decoction of yeast employed, Pasteur replaced the albuminoids in his fermentations by ammonium salts. In these solutions of pure sugar, with nothing but mineral additions, he demonstrated that the yeast grew and multiplied, and that its growth was accompanied by the conversion of the sugar into alcohol and carbonic anhydride, whilst similarly those totally distinct living corpuscles, to which he gave the name of *levure lactique*, proliferated in solutions of the same composition, and their multiplication was accompanied by the transformation of the sugar into lactic acid.

The amount of new experimental material collected by Pasteur in support of this vitalistic as opposed to the time-honoured chemical theory of fermentation is enormous, whilst his extraordinary power of seeing what others had failed to observe before him is again exemplified in his discovery of succinic acid and glycerine as invariable products of the alcoholic fermentation of sugar.

These researches, besides being of fundamental importance in throwing light upon one of the oldest, but hitherto obscurest, departments of scientific investigation, opened up an entirely new field of work, for with the inauguration by Pasteur of artificial culture solutions, that path was first indicated which has gradually expanded into the fascinating science of Bacteriology.

Whilst busily immersed in his researches on fermentation at Lille,

Pasteur received the intelligence, in October, 1857, that he had been appointed Director of Scientific Studies at the École Normale in Paris. There being no scientific laboratory attached to the post, Pasteur, unable to obtain any funds from the Government for such a purpose, improvised one out of a garret in the building and equipped it out of his own purse. Here he pursued with unabated energy the investigations which his removal from Lille had temporarily interrupted, and it was in the course of his further researches on fermentation that he made a discovery which, in respect of its wide and fundamental significance in relation to the economy of nature, is perhaps without an equal amongst his numerous and great achievements. This important discovery revealed the existence of living forms which grow, multiply, and develop mechanical energy in the absence of that oxygen which it had hitherto been regarded as one of the most far-reaching discoveries to have shown was indispensable for the whole living creation.

This new condition of existence, which he found pertained to the butyric ferment, Pasteur called *anaërobic,* as opposed to *aërobic* life, in which oxygen is essential to the continuance of life. This revolutionary discovery raised a perfect storm of opposition, and Pasteur's attitude to his opponents is well exemplified by the following words of his own :—" Whether the progress of science makes of this vibrio a plant or an animal it matters little, but it is a living thing, endowed with movement, which lives without air, and is a ferment."

This anaërobic life of the butyric ferment was not allowed to remain an isolated observation without bearing on other facts; but, on the contrary, its relationship to other known facts was at once discerned by Pasteur, who already in the same year, 1861, makes another communication to the Academy of Sciences, in which he develops in outline that celebrated theory of fermentation which has served to stimulate so many valuable researches.

Pasteur in his investigation of fermentative phenomena had thus by the year 1861 shown, firstly, the worthlessness of the form of words by means of which Liebig and the chemists of the time sought to banish all biological considerations from the study of these questions. Secondly, he had worked out a method of scientifically attacking these problems, in which, for the first time, both the chemical and biological aspects of the subject received their due share of attention. Thirdly and finally, by the systematic use of this new method of investigating fermentation phenomena, he had discovered the possibility of life without air, and had collected sufficient experimental data to venture on a new theory of fermentation.

The further researches which the new theory of fermentation stimulated were deferred for some years in consequence of his attention being directed to certain phenomena closely related to fermenta-

tion, and indeed demanding a full and final explanation before satisfactory progress could be made in this direction.

The researches to which we find Pasteur next devoting himself were directed to the settlement of the much vexed question—Can life originate spontaneously? It is impossible here to describe the history of this momentous controversy, but so unsatisfactory was the state of scientific opinion on this question in 1860 that the Academy of Sciences gave as a subject for a prize competition: "Essayer, par des expériences bien faites, de jeter un jour nouveau sur la question des générations spontanées." It is at this moment that Pasteur enters the lists, and the circumstance that we have for more than twenty years heard nothing of the doctrine of spontaneous generation is due to the effectual manner in which he successively hurled into the dust the several champions who appeared on its behalf in the intellectual tournament which followed.

In looking back upon this period of Pasteur's career, one is disposed to regret that his great powers should have been so long absorbed in this work of exterminating a mere superstition, but, as a matter of fact, much good came of this crusade in a number of ways. Incidentally, experiments, which have now become classical, were made on the distribution of micro-organisms in our surroundings such as air and water, whilst healthy urine and the blood of normal animals were, in 1862 and 1863, shown to be free from microbes and capable of being preserved without alteration for an indefinite period of time, provided they were collected with suitable precautions. Van der Broeck had, indeed, already, in 1857 and 1858, proved that grape-juice, white and yolk of egg, gall, urine, and arterial blood, if suitably collected, could be preserved without change in their natural condition, whilst subsequently sterile milk in its natural condition was obtained direct from the udder by Lister, Cheyne, Meissner, and others. The spontaneous generation controversy was, moreover, highly fertile in developing the general methods of bacteriological research, and many of the most familiar operations employed in the study of micro-organisms date from this period.

It is almost needless to say that the prize offered for researches throwing light on the question of spontaneous generation was awarded to Pasteur, and in 1862, at the age of forty, he was elected a member of the Academy of Sciences.

Pasteur now returned to his fermentation studies, and about this time we find him delivering an address to the vinegar manufacturers of Orleans, an address which has since become famous by reason of the important revelations which it contained concerning the production of vinegar by new methods. He had shown that the conversion of wine into vinegar is the work of a minute rod-like organism, which he called *Mycoderma aceti*, and he was now able to

point out to these manufacturers that instead of waiting the cus-
tomary two or three months for the completion of the process, the
vinegar could be elaborated in from eight to ten days by simply
exposing the vats containing the mixture of wine and vinegar to a
temperature of from 20 to 25° C., and sowing on the surface a small
quantity of this organism. As in the case of the alcoholic fermenta-
tion, so in that of the vinegar or acetic fermentation, Pasteur was
neither the first to discover the process, nor the first to see the living
ferment, nor yet even the first to connect the process with the life of
the micro-organism. The chemical change involved and the part
played by oxygen in the souring of wine were already indicated by
Lavoisier; the process was ascribed to catalysis, or contact action by
Berzelius in 1829. The familiar skin which forms on the surface of
the acetifying liquid was already named *mycoderma* by Persoon in
1822, and the bacterial cells of which this pellicle is composed were
seen and actually described under the name of *Ulvina aceti* by
Kützing in 1837, who even suspected a connexion between the life of
the organisms and the vinegar process.

But it is here that Pasteur stands out in such bold relief from so
many other distinguished *savants* of the century, for by building up
his discoveries on a solid rock of scientific experiment they have with-
stood those " whips and scorns of time " which have often succeeded
in demolishing the less successfully raised structures of his pre-
decessors.

In perusing the terse summaries in which Pasteur records his
labours on the acetic fermentation in the ' Comptes Rendus,' the
breadth and scope of the view which he takes of the phenomena
before him at once impress the reader, whilst the alertness of his
mind to developments, which even now are only partially realised, is
not less remarkable.

In placing the vinegar process on a sound scientific basis, Pasteur
had obviously already broached the subject of the "maladies des
vins," for that the souring of wine is one of the most wide-spread
ills to which it is subject, is surely well known to all possessing even
the most modest of cellars. What more natural, therefore, than
that Pasteur should conceive that those other and more mysterious
deteriorations which wines so frequently undergo, might receive a
rational explanation by the application of the same methods which
had elucidated the vinegar process? Nor were these methods found
wanting in dealing with the diseases of wine, for whilst in healthy
or normal wines Pasteur found only yeast cells, in all those wines
which connoisseurs condemned as diseased he found other micro-
organisms as well, and the nature of these micro-organisms was
found to vary according to the complaint with which the wine was
charged. Although a number of different bacteria connected with,

these several maladies of wine were described and figured by Pasteur, it must not be supposed that the mechanism of these processes was investigated with anything like the completeness of the acetification process. A large amount of work still remains to be done in connexion with these more obscure difficulties which attend the production of soured wine, but to Pasteur is due the broad explanation of these phenomena as dependent upon the presence of foreign organisms, and the further elucidation of the subject is chiefly a matter of laborious detail.

Pasteur, however, did not rest content with having discovered the cause of the deterioration of wine; he at once set to work to find out a means for its prevention, and in the first instance he sought to suppress the mischievous vitality of the micro-organisms present in wine by the addition of antiseptics. These experiments were not, however, satisfactory, and he, therefore, had recourse to heating the wine, and in this manner effecting the destruction of, or ensuring the paralysis of, the micro-organisms which produced the undesirable changes in its quality. This heating or partial sterilisation of a liquid—for the temperature employed was far below boiling, and was only designed to paralyse the activity of the micro-organisms present and not necessarily to destroy them—was first applied by Pasteur, and is now generally known as "pasteurisation." It has of late been largely and successfully employed in connexion with wine, beer, milk, cream, and other food materials of a perishable nature.

Pasteur's fermentation studies were, however, interrupted by an incidental investigation into which he was, so to speak, forced by his superiors, but which proved an admirable preparatory school for those great labours on micro-organisms and disease which have rendered his name a household word throughout the civilised world.

In 1865 the silkworm culture, which forms such an important industry in the south of France, was threatened with ruin in consequence of a most disastrous disease having made its appearance amongst the worms. A commission of inquiry was appointed, of which Dumas was made chairman, and he at once turned to Pasteur for assistance, requesting him to undertake the scientific investigation of the scourge which had wrought such misery amongst silkworm proprietors, reducing a successful and wealthy industry to the verge of destruction. Pasteur was very loath to leave his researches on fermentation and urged his entire ignorance of the subject, but Dumas would not listen to any plea as to his incapacity to undertake the work. "Tant mieux," he replied to Pasteur, "vous n'aurez d'idées que celles qui vous viendront de vos propres observations."

During five years Pasteur was unceasingly engaged in studying

the diseases of silk-worms, for he found that it was not only *pébrine* which decimated the silk-worm litters, but that another and totally distinct disease, *flacherie*, was also responsible for an enormous mortality amongst the worms. For five years then Pasteur was absorbed in unravelling these two diseases, and in discovering the best means for their prevention, and the vast amount of work which he accomplished in this connexion can only be approximately estimated by studying the two splendid volumes entitled 'Maladie des Vers à Soie,' published in 1870.

The opposition, the criticism, and the relentless scepticism with which the researches on silk-worm diseases and their prevention were received at the time, in a measure foreshadowed the bitterness of the conflict in which he was subsequently to become engaged in defending his investigations on the treatment of rabies.

The mental strain of his work told heavily upon Pasteur, and before he had been able to put the last and finishing touches, as it were, to his investigations, he was struck down with a severe attack of paralysis, from which his recovery was at first despaired of. Throughout his illness, however, his mind remained perfectly clear, but he never subsequently recovered the full use of his limbs.

The war of 1870 plunged him into deep despair, for Pasteur was perhaps an even more ardent patriot than *savant*, and for a time he seemed completely crushed, and unable to take up the thread of his researches. Prevented from returning to Paris on account of the Commune, he gladly availed himself of the offer of his old pupil, Duclaux, to come and work in his laboratory at Clermont Ferrand. Here he commenced those classical researches on the diseases of beer, which had for their object such an illumination of the brewing industry as would enable France to produce malt liquors of equal value and excellence to those for which her hereditary enemy across the Rhine had so long been pre-eminent. These researches were in 1876 collected and printed in a single volume, bearing the title ' Études sur la Bière,' which is unquestionably the best known of all Pasteur's publications in this country, and which has been translated into English. Throughout this volume Pasteur shows unmistakable signs that his thoughts and ideas were bearing in the direction of the applications which his methods and discoveries might have in the interpretation and treatment of the phenomena of disease. We find him making the significant suggestion that "l'étiologie des maladies contagieuses est peut-être à la veille d'en recevoir une lumière inattendue." Indeed, his researches *had* already borne fruit in many of the directions indicated by the above words, for Rayer and Davaine were encouraged to once more approach the investigation of anthrax, whilst we know that as early as 1865 Lister, then a surgeon in Glasgow, began his work in antiseptic surgery, based entirely on Pasteur's

brilliant researches, which demonstrated the truth of the germ
theory of putrefaction.

Pasteur followed with the closest attention and deepest interest
the developments of his ideas at the hands of the medical world, but
hesitated himself to undertake the responsibility of their application
to the study of infective diseases. " I am neither doctor nor veterin-
ary surgeon," he declares with modest diffidence. He was, how-
ever, in reality equipped as no man before had ever been, not only
with the necessary experience and training accumulated during
many years of devotion to original research, but also with the
scientific machinery necessary for the successful conduct of such
investigations, having brought his technique and methods to a pitch
of refinement and perfection which had never been equalled before.

Although fifty-five years of age, it is perhaps not surprising then
to find Pasteur ultimately devoting himself with all the enthusiasm
of youth to the study of pathological phenomena, the first subject
which attracted his attention being the well known infectious cattle
disease called anthrax.

We cannot here enter into the details of the history of this investi-
gation, but suffice it to say that the work of Rayer and Davaine, of
Pollender, of Delafond, of Koch, brilliant as the researches of these
investigators had been, had not succeeded in convincing the medical
and scientific world that the virus of anthrax was identical with the
so-called rodlets seen by Davaine in anthrax infected blood, and that
these rodlets, and not "the globules and plasma side by side with
them," constituted the real agents of the disease. The incontestable·
proof of the truth of this new doctrine was left for Pasteur to furnish,
and we know that he succeeded in removing all doubt on the question
of the etiology of anthrax once and for all. But more than this, in con-
junction with his assistants, Joubert and Chamberland, he discovered
another pathogenic micro-organism, the bacillus of malignant œdema,
an organism giving rise tò a deadly septicæmia, which in its anaërobic
mode of life much resembles the butyric ferment which Pasteur had
so successfully studied in 1861.

Having thus turned his attention to pathological researches,
Pasteur cast about him in all directions to obtain material, even
walking the hospitals and heroically overcoming his instinctive
antipathy to the sight of suffering and distress. But the occupation
of discovering pathogenic bacteria could not permanently engross
Pasteur's attention, and his mind's eye had long been riveted on that
great achievement of Jenner's which towers in royal isolation above
the plains of the medical history of centuries.

" Il faut immuniser contre les maladies infectieuses dont nous
cultivons les virus," was Pasteur's constant cry to his assistants at
this time. Haunted by this idea, Dr. Roux tells us how, during the

busy period which preceded the discovery of the attenuation of viruses, numbers of impossible experiments were gravely discussed amongst them, to be only laughed over the following day.

Yet another living virus must, however, be mentioned, which Pasteur successfully identified in these earliest days of his pathological researches, this was the specific agent of fowl cholera, the bacillus *Choleræ gallinarum*.

True it is that Perducito, in 1878, and Toussaint in the following year, had discovered and described the presence of this particular micro-organism in the blood of birds afflicted with this malady, but Pasteur was able to carry these observations a great and important step farther, for he succeeded in separating out this bacillus, and in cultivating it outside the animal's body, and in artificially inducing the disease in other fowls by the inoculation of such cultures. Interesting and important as was this achievement, it was destined to become of yet greater significance, for it was in the study of these fowl cholera organisms that Pasteur made that epoch-making discovery of the attenuation of virus and the artificial production of immunity.

On returning to his laboratory from a holiday which had interrupted his researches on fowl cholera, Pasteur, to his dismay, found all his cultivations of the microbes of this disease either dead or nearly so, and that many of the animals which he inoculated with these exhausted cultures appeared to suffer no ill effects whatever, a condition which contrasted painfully, from the experimenter's point of view, with the unerring fatal termination which had accompanied such inoculations before the vacation. Having at length succeeded in obtaining a virulent culture, the idea occurred to Pasteur of re-inoculating the birds which had survived the previous treatment with the exhausted cultures. What was his astonishment on seeing that these birds resisted the attack of the virulent organisms which proved rapidly fatal to those which had undergone no previous inoculation with the exhausted cultures. Convinced that this was no chance circumstance, but that he was here face to face with an entirely new phenomenon, Pasteur repeated the experiment in various ways, and found that he had indeed realised his great ambition of "immunising against an infectious disease, of which they cultivated the virus," and the microbe, which had hitherto only proved a malignant foe, was constrained to become the beneficent protector of its prey.

Numerous investigations were now made to determine upon what factors this conversion of the virus into a vaccine depended, and before long Pasteur was able to announce that the secret lay in the prolonged action of the air upon the culture at a suitable temperature. It was in the following year, 1881, on the occasion

of the International Medical Congress in London, that Pasteur, discussing his latest discoveries in the domain of immunity, paid a graceful tribute to his great predecessor of nearly a century before in this direction: "J'ai prêté à l'expression de vaccination une extension que la science, je l'espère, consacrera comme un hommage au mérite et aux immenses services rendus par un des plus grands hommes de l'Angleterre, Jenner." From this time onwards Pasteur's attention became concentrated upon the artificial production of attenuated viruses or vaccines, and he set to work to prepare the vaccine of anthrax. The simple method which had proved so successful in the case of the fowl cholera virus had, however, to be modified in that of anthrax, for the bacilli of anthrax, unlike those of fowl cholera, possess the property of producing spores, so that instead of exposure to the air diminishing the virulence of the culture, it did but serve to afford the bacilli of anthrax an opportunity of giving rise to spores, in which condition the virus is far more persistent and far more hardy than in the bacillar form.

It was necessary, therefore, to produce the anthrax vaccine by preventing the production of spores and then ageing the cultivations containing only the bacillar forms. This was accomplished by keeping the cultures at 42—43° C., at which temperature no spores are formed, and according to the length of time to which the cultures are thus exposed more and more attenuated viruses or vaccines of anthrax are obtained.

The gain to France from the application of Pasteur's method of vaccinating animals against anthrax has been estimated by M. Chamberland at 5,000,000 francs in respect of the lives of sheep which have been saved, and 2,000,000 francs for horned cattle. During the ten years 1884 to 1894 it is stated that as many as 3,400,000 sheep had been vaccinated with a mortality of 1 per cent., and 438,000 horned cattle, with a mortality of 3 per thousand.

Pasteur also showed that whilst the bacillus of anthrax can be rendered artificially less virulent, it can also become artificially endowed by suitable treatment with an increased virulence. A discovery which might at first sight appear to be of purely scientific interest, but which has in due time been found to possess an enormous practical importance.

Graduated vaccines were also obtained of the microbe giving rise to the disease known as *Rouget de porc*, or swine measles.

And now we come to Pasteur's last and crowning achievement, the prevention of disease in man—his discovery of a cure for rabies or hydrophobia.

Here again the limits of space prevent our entering into the details of this truly heroic struggle of the *savant* with the secrets of nature. During five years, for Pasteur's researches on rabies were

commenced already in 1880, and his first memoir on the subject was not published until the year 1885, during five years the great master was engaged with his faithful assistants in elaborating and perfecting a method for the prevention of rabies in human beings. The results of these years of work may be briefly summarised in the following concise words of Roux :—" The spinal marrows of rabid rabbits when exposed to the action of the air, in a dry atmosphere, become desiccated, and lose their activity. After fourteen days the virus is attenuated to such an extent that it is harmless, even in the largest doses. A dog receiving this fourteen-days-old marrow, then on the following day thirteen-days' marrow, then that of twelve days, and so on up to the fresh spinal cord itself does not take rabies, but has become immune to it. Inoculated in the eye or in the brain with the strongest virus it remains well. In fifteen days, therefore, it is possible to confer immunity upon an animal from rabies. Now, human beings bitten by mad dogs do not usually develop rabies until a month, or even longer, after the bite. This time of incubation can be utilised to render the person bitten refractory."

Still, however, the most difficult, the most perilous task remained to be accomplished—the application of the knowledge and experience thus acquired to the prevention of rabies in man. It was in July, 1885, that this momentous step was taken, and in October of the same year, Pasteur communicated that celebrated memoir to the Academy of Sciences in which he described the results of what he modestly designated a " tentative heureuse." A profound impression was produced by this successful result, and wounded persons soon streamed in from all parts, and during the following year as many as 2,682 individuals were treated, each of whom on an average, received between fifteen and twenty inoculations. Nearly 20,000 persons have now undergone Pasteur's anti-rabic treatment at the Paris Institute, and the mortality has been less than 5 per 1,000.

This magnificent triumph was not easily secured, and it was amidst the most determined opposition at home and abroad that Pasteur had to fight over two years for the public recognition of this great discovery; the effect of this strain told terribly upon his health, and Pasteur was obliged to give up his life in the laboratory, although he never ceased, down to the very last days of his life, to take the keenest interest in all the investigations which were being carried out in that great institute which bears his name, and which is the public expression of gratitude for the magnificent services to suffering humanity with which his name will for ever be associated.

It was in November, 1888, that the Institut Pasteur was opened with a brilliant ceremonial, by the President of the Republic, and

we have only to glance at the *Annales* of the Institute, to see how amply it has since justified the great hopes with which its inauguration was accompanied.

If Pasteur's life was darkened by struggles, it was equally illuminated with brilliant victories, which he not only prized on account of the vindication of the truths for which he fought, and for the love of science, but also for the lustre which they shed upon his beloved country.

"Si la science n'a pas de patrie," has said Pasteur, "l'homme de science doit en avoir une, et c'est à elle qu'il doit reporter l'influence que ses travaux peuvent avoir dans le monde." It was in this sense that Pasteur participated with such profound feelings of emotion and gratitude in the magnificent celebration of his jubilee which took place on December 27, 1892, in commemoration of his seventieth birthday. Dr. Roux has told us how this unique ceremonial, in which nations forgot politics and joined together to honour and gratefully acknowledge the monumental labours of one of the greatest men of science of the century, profoundly moved Pasteur. "Puis Pasteur ne vécut plus que par l'amour des siens; il fallait les soins et toute l'affection dont il était entouré pour prolonger sa faiblesse." It was in the autumn of 1895 that the news of Pasteur's grave state of health gave cause for universal anxiety, and on the 28th of September he passed away at Villeneuve-l'Étang, near Garches. A mausoleum erected by the Pasteur family in the Institut Pasteur affords a last resting place to the mortal remains of him who

> "Defender of the living, now shall keep
> His slumber in the arsenal of life."

Pasteur has left behind him nearly two hundred notes and memoirs inserted in the 'Annales de Chimie et de Physique,' in the 'Comptes Rendus,' in the "Recueil des Savants Étrangers," &c., besides several works published separately.

The honours which were showered upon him by numerous foreign public and scientific bodies, besides those bestowed upon him by his own country, are too many for record here; mention should, however, be made that the Royal Society, which had awarded him the Rumford Medal in 1856, conferred upon him the Copley Medal in 1874, and in 1869 elected him a Foreign Member of the Society.

<div align="right">P. F. F.</div>

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ERRATUM.

Page vii, line 13 from bottom. *For* 1870, *read* 1874.

END OF THE SIXTY-SECOND VOLUME.